JANUARY 1960

Current Anthropology

A WORLD JOURNAL OF THE SCIENCES OF MAN

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CURRENT ANTHROPOLOGY is a co-operating group of scholars who interchange knowledge and ideas by means of this journal. These scholars are called "Associates in CURRENT ANTHROPOLOGY" and have the obligation to foster the full and free world-wide interchange of knowledge in the sciences of man centering around physical anthropology, prehistory, archaeology, linguistics, folklore, ethnology, and social anthropology. Associates by supplying information and ideas to all others are therefore assured also of receiving what they need to pursue their own special interests. The concept of an "Associate" is extended also to institutions in the central fields which provide information through CURRENT ANTHROPOLOGY.

The journal CURRENT ANTHROPOLOGY, though initially the means of intercommunication among Associates, is a public document making it possible for interested scholars in other fields, for students, and for others, to know what is going on in the sciences of man. Therefore, subscriptions are accepted from anybody. Associates receive the journal for a smaller fee because their participation means additional expenses for postage and other services. Students in the central disciplines, or scholars in closely related fields, who therefore need to read CURRENT ANTHROPOLOGY, may be recommended by Associates to subscribe at a special rate.

The value to all the sciences of man of facilities for such world-wide scholarly interchange has been recognized by the Wenner-Gren Foundation since its founding in 1941. The International Symposium of 1952 (resulting in the publication of *Anthropology Today*, *Appraisal of Anthropology Today*, and the *International Directory of Anthropological Institutions*) was followed by the publication of an experimental *Yearbook of Anthropology*, one issue of which appeared in 1955. Part of this was republished in 1956 under the title *Current Anthropology*, which is the origin of the title of this journal. In 1957 the Foundation, seeking means to pursue these ends, established CURRENT ANTHROPOLOGY. Since that time the many possible forms CURRENT ANTHROPOLOGY might take have been discussed in conference and by correspondence with scholars all over the world.

In August, 1958, the conference which inaugurated Burg Wartenstein (in Austria) as a European conference center of the Wenner-Gren Foundation was devoted to this subject. It laid down these broad principles of CURRENT ANTHROPOLOGY:

1. It should be as broad and open as the problems of the changing sciences of man require, bringing together the widest variety of relevant ideas and data, and extending and facilitating intercommunication among students of man wherever in the world they are working.

2. It should be unitary, a single set of cross-cutting materials available to all. All students of the sciences of man should be speaking to one another on the same pages in the same language. Though no scholar can be equally interested in all things, CURRENT ANTHROPOLOGY should not prejudge where the individual's interests will carry him.

3. It should provide communication that is both fast and convenient, a single, common center where scholars can with a minimum of duplication of effort share knowledge of their current activities.

These principles, during the following year, were accepted with enthusiasm at meetings in all parts of the world, and were developed into the present plan with the help of at least 1,000 colleagues. CURRENT ANTHROPOLOGY is a bimonthly journal in English of, by, and for the scholars of the world who pursue the anthropological sciences including physical anthropology, ethnology, folklore, linguistics, social anthropology, prehistory, and all their sub- and related sciences by whatever names.

CURRENT ANTHROPOLOGY publishes two classes of materials, "Review" articles and "News and Reference" materials.

A Review Article is a major review of current knowledge (and its bibliography) in any relevant subject matter, or of the literature in some broad area of current interest. It is a guide by a specialist of the subject addressed to specialists in other fields. It should therefore treat a subject of interest to a broad segment of anthropologists usually comprising several of the subdisciplines, and for those of different scholarly traditions; and which are either new considerations of traditional subjects or preferably subjects of new and growing interest. New material at the growing points of anthropology and new evaluations are preferred to syntheses of what has become well known, but the new should be placed in the context of the known. Therefore, also, no matter how technical the subject, an article should explain itself and be written in language that will be followed by scholars in other specialties.

Each CURRENT ANTHROPOLOGY Review Article will be handled according to what we call "CA☆ treatment" and thus will be the nucleus for intercommunication among specialists in the area covered by that paper. After a paper has been read and provisionally accepted, it will be duplicated and sent to a list of readers. This list will include names suggested by the author and will have two general categories of people: (1) readers who are also experts in the topic under consideration, and (2) readers whose interests are at the edge of the material; people who treat it either as part of a larger whole, or as the whole of which they are primarily concerned with the parts. In both cases the readers may add material, argue the interpretation, or say nothing. In every case: (*a*) readers' comments will be sent to the author for consideration and action; (*b*) the Editor will review this use of the comments, and (*c*) consult with the readers and author if he sees any problems. This plan seems to be a promising way to facilitate intercommunication. By sending each paper to a variety of people, we shall have a different level of accuracy and comprehensiveness, and a greater opportunity to answer the questions which arise at the edges of our field.

"News" is necessarily limited to items of importance and general interest; but items of importance to fewer Associates may be published in brief form where appropriate.

"Reference" materials include systematic guides to materials of general interest—a bibliography of published bibliographies, guides to guides of research collections, etc., directories of scientists and scientific institutions in all of our fields; lists of Ph.D. dissertations and research in progress; and whatever other standard information seems most useful to facilitate the work of all.

CURRENT ANTHROPOLOGY will eventu-

Continued on inside back cover

4-2-22

Our Readers Write

In the context of consideration of subjects of new interest, review articles on the problems and materials relating to geographic regions relatively new to anthropologists might also be included. I am thinking of "under-studied" areas such as the Middle East, Central Asia, Nepal, etc., where so far little anthropological work has been done. Prospective field workers require some orientation.

CHRISTOPH VON FÜRER-HAIMENDORF

[Suggest] dealing with the revival of certain older ideas and approaches—e.g., evolutionism; environmentalism; biological basis of behavior. In my opinion, one of the most significant aspects of contemporary anthropology, at least its historical aspect, is precisely this revival of 19th and early 20th century approaches and problems—to create, perhaps, the new "general anthropology." I would think of the task, . . . as a contribution to the history of "current anthropology."

JOHN W. BENNETT

The area between the Nile valley and Lake Chad, very little known because difficult of access now, was probably much more habitable when civilization began to develop in the neolithic period and this area if explored will probably produce the solution to some of the problems facing prehistorians now in Egypt and the Sudan. Exploration should be encouraged. A clear and up-to-date summary of prehistory in the Fertile Crescent (Palestine, Syria, Iraq and Persia) from 5000–10000 B.P. would be welcome to prehistorians working on Egypt, for the influence of Asia at this time was undoubtedly considerable. I am working up surface finds made on an expedition south and west of Kufra, which concentrated on the Wanyanga lakes area, and on the Amazon stone quarries of Eghel Zumma (NE Tibesti). ANTHONY J. ARKELL

Badly needed: a reassessment of the comparative method in Anthropology.

ALLAN HOLMBERG

Social and psychological aspects of Shammanism.

URSULA KNOLL-GREILING

An important help to research would be the publication little by little of a corpus of ethnographic specimens in museums and private collections, each item being illustrated by a photograph or a line drawing. JEAN GUIART

. . . a department of correspondence (letters). THOMAS HORACE EVANS

. . . reports on current research activities of major importance since 1950 (i.e., since the 3rd edition of the *International Directory of Anthropologists* was published). BRANIMIR BRATANIĆ

Delighted with prospects for publication of current news and reference materials (filling a long-standing and unfortunate gap in anthropological literature). Would it be feasible to incorporate in CA an ongoing revision of *International Directory of Anthropological Institutions?*

DWIGHT B. HEATH

I think we should have a few articles by non-anthropologists criticizing special topical approaches in anthropology. GEORGE D. SPINDLER

. . . cross-cultural interests in areas of urbanism and industrialism.

PAUL MEADOWS

. . . a general article on ethnomusicology and its relationship to anthropology. ALAN P. MERRIAM

. . . Asian anthropology, particularly relatively unexplored fields like the sociology of Buddhism and Hinduism.

GANANATH OBEYESEKERE

. . . articles on high level synthesis and interpretation of archaeological subject matter and concerted attempt to explore the problems and possibilities of the overlap of ethnology and archaeology at the contact period.

REYNOLD J. RUPPE

It seems to me that a particular effort should be made to make accessible to researchers of the different countries the interesting works which appear in languages of limited circulation.

ISAC CHIVA

. . . something on Soviet linguistics (they are so very busy on Indian languages) can be included—so far we have no contact with that research.

WILLIAM MCCORMACK

Permanent sections for each major geographical area. EMILIO ESTRADA

Review articles such as, in medicine, "Recent advances in Dermatology" will really not quite do. There are too many areas of fundamental disagreement, so that finding really "neutral" reviewers might often mean finding a pretty third-rate one. I would suggest that such surveys come usually in pairs, by specialists who follow the recent special literature but who have differing points of view. CHARLES F. HOCKETT

. . . commission articles from various countries which summarize the focus of interest of anthropologists—that is, the kinds of problems and approaches which are orienting the work of anthropologists in countries with which we have minimum contact because of language, political and other barriers. This will, of course, begin to emerge from the character of specific articles over time; but I think that some special attention to critical and evaluative surveys (on the order of those in *Anthropology Today*) at the beginning would be of immense interest to social scientists at this time. WARREN D'AZEVEDO

Important to include significant new techniques and new approaches and new discoveries even if published elsewhere in hard-to-get journal or difficult foreign language, or journal not normally available to a large section of CURRENT ANTHROPOLOGY readers. Reprint of some old basic papers now unobtainable also of great use.

JOHN TALBOT ROBINSON

Include more works of Soviet and Chinese authors.

CARLOS ALONSO DEL REAL

My preference is towards symposia-type issues: such as *African Political Systems*, or *Village India;* or *Anthropology Today*, where a group of contributors have first exchanged views at conferences or seminars allowing room, however, for others to contribute to the issue covering the particular symposium. The above need not rule out the special, or unusual, or outstanding articles that ought to be read at the time; it need not rule out news and views, etc.

GITEL P. STEED

Special issues devoted to the analysis of one many-faceted problem.

PAUL R. DUCEY

. . . a summary article on Theories of Kinship and scientific classificatory concepts & terms employed in kinship studies.

Re: Peoples of the World. I suggest one or more articles specifically focused on emergent (if you will, new national) cultures. For instance: The (British) West Indies; . . . Ghana, Nigeria, etc.; Ceylon; Indonesia, etc. BEATE R. SALZ

. . . palynology should be included as it has proved an important tool.
EDUARD M. VAN ZINDEREN BAKKER

. . . review of field techniques in cultural anthropology . . . particularly methods and problems of interviewing. Although we have many discussions of testing and recording (TAT's and tapes) we have no survey of general techniques of conducting interviews in the field. Discussions of interviewing techniques by non-anthropologists are available but do not cover the problems of ethnological field work. . . . In any case I think a paper on basic ethnological field techniques would be valuable and important. ALFRED G. SMITH

Special numbers dealing with one single topic in different communities (e.g., the impact of Western civilization on underdeveloped communities), or with the different aspects of life in one specific area (e.g., Mediterranean communities). AHMED MOSTAFA ABOU-ZEID

It might be interesting to have "special issue" sometimes on a particular field by various different specialists of different nationalities. CHIE NAKANE

Will there not be abstracts or précis of articles in one or two other languages?
ETHEL BOISSERAIN

I fully endorse the program of CURRENT ANTHROPOLOGY. It is noteworthy that the social sciences as these are developed in the West are so culture-bound that their use in underdeveloped areas such as my country suffers from severe limitations and are highly misleading. This generalization can admit of only one significant exception, and that is anthropological science. Anthropology as it has developed is clearly a study of underdeveloped countries. I therefore would like to see in the pages of CURRENT ANTHROPOLOGY appropriate models that will explain problems of economic poverty and underdevelopment.
JOSE SOBERANO

How about anthropological area researchers drawing upon resources of public opinion and communications survey procedures? JAMES SILVERBERG

This should result in the very much needed better acquaintance among the workers in the various Sciences of Man. . . . The choice of English reminds me of the interesting fact that more than 50 years ago the great French botanist, A. de Candolle, outlined the reasons why English should be the world language. DONALD D. BRAND

Facilities should be provided for visiting anthropologists to make personal contacts either way; results of field work and latest discoveries should be publicised; new methods and techniques should be made known; amateur anthropologists should be encouraged and assisted but the professional anthropologists should be distinguished and due consideration given.
NANDADEVA WIJESEKERA

Emphasis on "growing points" in anthropology wholly excellent. I favour space being made available for correspondence in CA for points grow partly by controversy. VICTOR TURNER

I hope the CA unites closely all scholars in so many different countries as well as those in many different areas of discipline, all related to a great human enterprise: the study of man.
HIROSHI WAGATSUMA

. . . a necessary project. Especially the "Want Ad" section, which seems to be an urgent need. CARL G. WIDSTRAND

. . . a dictionary of anthropology edited by AAA and CURRENT ANTHROPOLOGY.
MORIMITSU USHIJIMA

In the field of palaeolithic, the most important, I think, is publication of new material from good (or, if possible, the best) excavations, and establishment of *realistic* definitions of used terms. DENISE DE SONNEVILLE-BORDES

I hope that CURRENT ANTHROPOLOGY may make it a point to *be* current by publishing brief accounts from the field or shortly after return.
KENNETH MACGOWAN

International and regional conferences, international contacts, pre-publication news. FRITS PETER LISOWSKI

How about a "discussion section" for short contributions describing, commenting on, or criticizing such things as methods, aims, and philosophy in different fields of anthropology? Might include, or begin as, commentary on previously published major reviews. Particularly, comments from specialists in different branches might give important new viewpoints. BJÖRN KURTÉN

Regarding format, I urge that major articles be preceded by a brief abstract —as followed in such other professional journals as that of the Geological Society of America. RONALD J. MASON

Please keep publications on a level of inter-field understanding.
D. CROSS JENSEN

A great need would be served if CURRENT ANTHROPOLOGY concentrated on jargonless (British style) articles to keep persons who were trained in anthropology up to date theoretically and substantively. It would be pleasant to have a "readable" publication of use to anthropologist and non-"A" alike. I have great respect for the inherent clarity of the English language.
WILLIAM MORRISS CARSON

In the rapidly expanding field of blood group anthropology the Nuffield Blood Group Centre (for which I am largely responsible) serves as a world information centre. We attempt to collect and tabulate all available data, published or unpublished, and to make our records (apart from confidential unpublished data) available to any worker who needs them. I feel that in this field CURRENT ANTHROPOLOGY can help us in securing data and we can help CURRENT ANTHROPOLOGY by making the data available. (This is a field where the bibliographic problem is especially severe since records tend to be published in small local medical journals.)
ARTHUR E. MOURANT

. . . would it not be appropriate and forward-looking to include a section, even if minor, on "Educational Anthropology" or whatever you wish to term the developing cousin of educational sociology and educational psychology? This would be all the more timely in view of the current re-evaluations of American education, and the indications that anthropological perspectives may prove invaluable.
WILLIAM SAYRES

Two suggestions: Let's not lose sight of the realm of culture process as so many anthropologists seem to be doing, today, at least in terms of current writing.

Don't you think that it might be profitable to have a look at the border zone between anthropology and history? . . . historians could profit from exposure to some of the fundamental concepts of anthropology, and history can certainly provide the anthropologist with a vast store of documented situations in which to observe culture in action over relatively long periods.
DONALD J. LEHMER

Lexicostatistics So Far

by D. H. Hymes

The anthropological historian is performing a task of fundamental human importance: it is but befitting to the dignity of the human race that it should know as much of its own past as possible.—C. F. Hockett[1]

INTRODUCTION

In the last decade glottochronology has excited international interest and acquired a literature of its own. To the anthropologist it promises a measure of time depth for language families without documented history, and yet another linguistic example of regularity in cultural phenomena.

It is tempting to think of reasons why glottochronology should not work, and some find it hard to accept the fact that it can. It is tempting for an anthropologist to use even provisional findings of linguistic relationship and time depth, and some find it hard not to accept them uncritically. Either may be unfortunate. Extreme skepticism delays the maturity of glottochronology, and of lexicostatistics, the field of which it is so far a part. Rash use of provisional results may give way to rash disillusionment.

In this paper I shall try to give balanced appraisal to lexicostatistics, as it has and as it may be developed.[2] The focus will be on glottochronology, the best-known lexicostatistic method, but other topics also will be brought into view. The rest of this introduction will give a thumbnail sketch of glottochronology, define terms, and touch on a possible confusion.

Dell H. Hymes is Assistant Professor of Social Anthropology and Linguistics at Harvard University (Cambridge, Mass., U.S.A.) Born in 1927, he was educated at Reed College (B.A., 1950), Indiana University (M.A., 1953; Ph.D., 1955), and the University of California at Los Angeles (1954–55).

Hymes has been concerned with historical anthropology especially in regard to American Indian languages and problems of method. In 1957 he wrote a survey paper for a Semana Linguistica on "Gloto-Cronologia y las Lenguas Oto-Mangues" sponsored by the Consejo de Lenguas Indigenas of Mexico. The present paper is a considerably changed and expanded version.

The present paper, submitted on May 8, 1959, was sent to seven scholars for CA☆ treatment. All seven responded: Harold E. Driver, Isidore Dyen, Henry A. Gleason, Jr., Sarah C. Gudschinsky, Shirō Hattori, Wilhelm Milke, and Morris Swadesh. Their contributions were incorporated by the author into text or notes, or as appended "Comments," and are indicated by a star (☆).

THUMBNAIL SKETCH OF GLOTTOCHRONOLOGY

Swadesh (1950, 1952), Lees (1953), and Swadesh (1955*b*) are fundamental references for glottochronology as it has been developed and practiced so far. Other papers, particularly those of Swadesh, contain discussions and applications, and as full as possible a bibliography is given at the end of this paper. So far the best exposition of the techniques for doing glottochronology is Gudschinsky (1956*b*); her article gives a complete example of cognate counting, calculation of minimum divergence time, and range of error, and includes relevant statistical apparatus. Hockett (1958: chap. 61) has a clear introduction to the method, Gleason (1955*a*: 343) a brief presentation. Gleason's *Workbook* (1955*b*: 87) provides a glottochronologic problem.

The question out of which glottochronology arises is that of the rate of change in language. The glottochronologic approach is based on the principle stated by Sapir: "The greater the degree of linguistic differentiation within a stock the greater is the period of time that must be assumed for the development of such differentiation" (1916: 76).[3]

If we could measure degree of differentiation, for two related languages, this would index the relative length of time that they had been diverging from their common ancestor. If such an index could be calibrated with chronological time, this would provide actual dating of the common ancestor: it would be glottochronology. The only glottochronologic method so far developed deals with differentiation in a part of vocabulary which is considered *basic*. It samples this part by a test list of meanings. The common, everyday equivalents for this list are obtained in two (or more) languages. There are two types of cases. If the languages are different stages in a single line of development, it is a *control case*. If the languages are the outcomes of different lines of development from a single ancestor, it is a *case of application*.

The glottochronologic method involves three principle variables: a rate of retention, a period of time, and the fraction of corresponding test-list equivalents in two languages that are cognate. In control cases, the length of time between the two languages or stages is

known, or can be estimated, and this fraction thus constitutes a rate of retention for that period of time. Rates of retention for a number of control cases can be computed and compared. (The time interval will differ from case to case, but can be converted into a rate per millenium.) If the retention rates for different control cases cluster within a narrow range, it is reasonable to compute an average retention rate, and to regard this average as having validity beyond the control cases themselves.

The first work found that retention rates did indeed cluster within a certain range. It "revealed that the fundamental everyday vocabulary of any language—as against the specialized or 'cultural' vocabulary—changes at a relatively constant rate" (Swadesh 1952: 452). In fact, this discovery surprised the investigators, who had hoped only for indices useful within particular language families. It is exciting in itself to discover an apparent regularity of cultural change, but the greatest excitement has been over the prospects of application.

In control cases, we have noted, the period of time and common fraction are known, the retention rate to be found. The interest is not in dating, but in regularity or diversity in this aspect of culture change. In cases of application, either the languages are without written history, or there is interest in time depths and sub-groupings that are not documented; in either event, time depth is unknown, but if a rate of retention can be assumed, time depth can be inferred. All that is required is to compute the number of pairs of corresponding items on the test list for which two related languages show cognate equivalents, and then to put this into an appropriate formula together with the retention rate, or to consult a nomograph curve or prepared table based on such a formula, such as that prepared by Leonardo Manrique, which is to appear in full in Arana (MS.*b*) and which appears in part in Swadesh (1959*c*). This opens up a new way to establish subgroups within a family of languages, and a heady prospect for integrating the results of genetic classification and lexical reconstruction with those of archaeology, ethnology, and culture history, to secure time perspective on the human past.

THE TERMS "GLOTTOCHRONOLOGY" AND "LEXICOSTATISTICS"

The terms "glottochronology" and "lexicostatistics" have often been used interchangeably. Recently several writers have proposed some sort of distinction between them (Gleason 1959; Hymes 1955, 1957*a*; Marshall 1956; Voegelin 1958). I shall now distinguish them according to a suggestion by Swadesh.☆

Glottochronology is the study of rate of change in language, and the use of the rate for historical inference, especially for the estimation of time depths and the use of such time depths to provide a pattern of internal relationships within a language family. *Lexicostatistics* is the study of vocabulary statistically for historical inference. The contribution that has given rise to both terms is a glottochronologic method which is also lexicostatistic. Glottochronology based on rate of change in sectors of language other than vocabulary is conceivable, and lexicostatistic methods that do not involve rates of change or time exist (e.g., Kroeber and Chretien 1937, 1939; Ellegård 1959; Gleason 1959, for atemporal subgrouping; Swadesh 1954*a*, 1956*a*, for lexicostatistic test of genetic relationship).

Lexicostatistics and glottochronology are thus best conceived as intersecting fields. Much of our focus in this paper is, strictly speaking, on lexicostatistic glottochronology, but since the context is clear, and no other such method is in use, we shall speak simply of "glottochronology."[4]

It is true that "lexicostatistics" might properly name any statistical study of vocabulary, rather than specifically historical studies, and one could speak of "historical lexicostatistics" if need be. However, the term "lexicostatistics" has already a specialized association with historical studies.

A POSSIBLE CONFUSION

It is important not to confuse rate of change in basic vocabulary with rate of change for a language as a whole. Trager (1955) has rightly insisted on this point. Glottochronology deals with rate of change only for basic vocabulary as sampled by a test list. The literature, however, contains statements such as "the glottochronological method assumes for the time being that all or most languages, whatever their kind or type, change at a pretty uniform, steady rate" (Kroeber 1955: 91); "The method is based upon the discovery that languages apparently change at a uniform rate" (Hoijer 1956*b*: 219); and, "Swadesh deduces probable speeds of language change for some non-European languages, largely upon a basis of well-founded estimates of speeds of change for the languages of very advanced societies" (Jacobs 1954: 53). Such statements may be convenient shorthand, but they may encourage specious refutation. Some take them literally, and it is easy to point to cases in which phonological or grammatical systems seem to have changed at quite different rates. In point of fact, a fruitful area of glottostatistics might be the study of differences in rate of change for different parts of language.

The major sections of this paper concern the basis of glottochronology, the application of glottochronology, the uses of lexicostatistics, and the strategy of lexicostatistics.

THE BASIS OF GLOTTOCHRONOLOGY

THE PROBLEMS of the basis of glottochronology are implicit in the formula $t = \log c / \log r$, and the instruction, solve for r. This is the formula used in control cases to determine the rate of retention (r) when the period of time between two stages of a language, and the fraction of common forms on the test list, are known. Criticism and discussion have been directed at the concepts "non-cultural" and "basic vocabulary"; at the validity of the test lists used; at the adequacy of the control cases; and at the validity of the retention rate. I shall discuss these in turn.

THE CONCEPTS OF "NON-CULTURAL" AND "BASIC" VOCABULARY

Because the items of the test list seem independent of any particular type of culture, they have been dubbed

"non-cultural." This usage has been criticized, and rightly so. All of language, including vocabulary, is a part of culture (e.g., Swadesh 1951*a*: 1–2). The usage was introduced within quotation marks ("... the fundamental everyday vocabulary of any language—as against the specialized or 'cultural' vocabulary . . ." [Swadesh 1952: 452]), but the quotation marks fell away. We should drop the usage altogether, and speak of "basic" and "non-basic" vocabulary.

The conception of vocabulary as polarized has a long and important history. One pole has been conceived as that at which gather words the referents of which are most nearly universal, which are most common, and which are most resistant to replacement. Thus, Whitney states that cultural borrowing may induce lexical borrowing, but "it cannot touch the most intimate fund of speech, the words significant of those ideas without whose designation no spoken tongue would be worthy of the name" (Whitney 1891: 197–98). Whitney specifies numerals, pronouns, and kin terms, and points out that the great amount of borrowing into English has "left the language of the common life among us—the nucleus of a vocabulary which the child first learns, and every English speaker uses every day, almost every hour—still overwhelmingly Saxon." He finds parallel cases as well (p. 198). A similar specification is made by Kent (1945: 15). Meillet observed that all words are not borrowed with equal facility, and that common words are more difficult to replace (1958: 84). He enlarged on this attribute in discussing the difficulty of proving genetic relationship in areas of the world where one had mostly only vocabulary to work with (for he considered grammatical facts the true basis of genetic proof): "If in such a case one observes a very large number of shared vocabulary items among certain languages, and if these concern the words least subject to borrowing, notably the verbs which indicate usual actions such as *go* and *come*, *drink* and *eat*, *live* and *die*, *hear* and *see*, *say* and *be silent*, etc., or adjectives such as *old* and *new*, *big* and *small*, *long* and *wide*, etc., it would be pure pedantry to refuse to make use of them" (1958: 94). For the antiquity in Indo-European work of such a hierarchical historical view of vocabulary, see the pioneer comparativist Schlegel's *Uber die Sprache und Weisheit der Indier* (p. 77, cited in Twaddell 1943: 151).

Outside Indo-European, a pioneer student of Austronesian (Malayo-Polynesian) languages, Sidney Ray, wrote: "A very important fact to observe is that the words which appear to be of cognate origin in Melanesian and Polynesian are those which denote the simplest conceptions, parts of the body, some relationships, natural objects, and a few of the simplest verbs and adjectives, and that these are words which are least likely to be loan words" (1919: 53). Discussing what is meant by genetic relationship, Bloomfield writes: "The resemblances appear especially in features that are common in everyday speech—in the commonest constructions and form-classes and in the intimate *basic vocabulary*"; and of Germanic examples, he writes "the resemblances are so many and they so thoroughly pervade the *basic vocabulary* and grammar, that neither accident nor borrowing will explain them" (1933: 298, 299; italics added). In a recent paper, Dyen resolves a difference of interpretation by "a fundamental procedure in the application of the comparative method," the use of basic vocabulary as criterion to discriminate inherited forms from borrowings; he refers to "that part of the vocabulary that is customarily called basic" (1956*a*: 85). Though he does not define or characterize it, Greenberg (1957: 39ff.) uses the concept of fundamental or basic vocabulary as an essential part of determining genetic relationship; like Dyen, he implies general recognition of what comprises basic vocabulary.

There can be little serious objection to recognizing a polarity of two broadly recognizable lexical sectors, opposed in terms of attributes of commonness, universality, and historical persistence or resistance to borrowing. The basic sector is that which enjoys these attributes to a high degree; the non-basic sector is that which has them to a low degree. It is not necessary to assume a sharp dichotomy between the two sectors. Probably there is a continuum between the two poles.

It remains that our general understanding of the notion of basic vocabulary is not matched by specific knowledge of what constitutes it in particular cases. Nor has the validity of our general understanding received much in the way of empirical test. The quotations from Whitney and Bloomfield point to the assumptions that: there are some things that are likely to be mentioned frequently in everyday speech; there are some things the names of which are likely to be learned early by a child, and, because of frequency, over-learned; there are some things for which there are likely to be words in every language; there are some meanings for which words are unlikely to be borrowed (a corollary of the preceding assumptions). It is a virtue of glottochronology that it has made the investigation of these assumptions the order of the day. Indeed, what one can say about glottochronology now, depends very much on efforts to develop and apply a glottochronologic test list, and it is on these efforts that our discussion will focus.

THE VALIDITY OF THE TEST LIST

In this section we will consider how well the test list now in use meets the criteria of frequency, universality, and persistence.

By way of preliminary, we should note some convergence as to what items of meaning pertain to the basic sector. In 1863, George Gibbs issued a list based on one by Albert Gallatin with a few changes by Horatio Hale; it contains 211 glosses, each in English, Spanish, French, and Latin. Allowing for a few slight variations in form, 102 of the items on the 200-item glottochronologic list are represented, and 73 of the items on the 100-item glottochronologic list. (Thus the percentage of agreement is greater on the revised list.) Most of the non-agreement is accounted for by a long sequence of numerals in the Gibbs list, a number of possessive kin-terms, and a number of items of material culture and animal and plant life specific to the American Indians. In the "Linguistic Appendix" to his *The American Race*, Brinton used a short word list to show relationships among a number of Latin American languages: "The words I have selected for the vocabulary are those

200-Item Test List (1952)

1. all
2. and
3. animal
4. ashes
5. at
6. back
7. bad
8. bark
9. because
10. belly
11. big
12. bird
13. bite
14. black
15. blood
16. blow
17. bone
18. breathe
19. burn
20. child
21. cloud
22. cold
23. come
24. count
25. cut
26. day
27. die
28. dig
29. dirty
30. dog
31. drink
32. dry
33. dull
34. dust
35. ear
36. earth
37. eat
38. egg
39. eye
40. fall
41. far
42. fat-grease
43. father
44. fear
45. feather
46. few
47. fight
48. fire
49. fish
50. five
52. flow
51. float
53. flower
54. fly
55. fog
56. foot
57. four
58. freeze
59. fruit
60. give
61. good
62. grass
63. green
64. guts
65. hair
66. hand
67. he
68. head
69. hear
70. heart
71. heavy
72. here
73. hit
74. hold-take
75. how
76. hunt
77. husband
78. I
79. ice
80. if
81. in
82. kill
83. know
84. lake
85. laugh
86. leaf
87. leftside
88. leg
89. lie
90. live
91. liver
92. long
93. louse
94. man-male
95. many
96. meat-flesh
97. mother
98. moun-tain
99. mouth
100. name
101. narrow
102. near
103. neck
104. new
105. night
106. nose
107. not
108. old
109. one
110. other
111. person
112. play
113. pull
114. push
115. rain
116. red
117. right-cor-rect
118. right-side
119. river
120. road
121. root
122. rope
123. rotten
124. rub
125. salt
126. sand
127. say
128. scratch
129. sea
130. see
131. seed
132. sew
133. sharp
134. short
135. sing
136. sit
137. skin
138. sky
139. sleep
140. small
141. smell
142. smoke
143. smooth
144. snake
145. snow
146. some
147. spit
148. split
149. squeeze
150. stab-pierce
151. stand
152. star
153. stick
154. stone
155. straight
156. suck
157. sun
158. swell
159. swim
160. tail
161. that
162. there
163. they
164. thick
165. thin
166. think
167. this
168. thou
169. three
170. throw
171. tie
172. tongue
173. tooth
174. tree
175. turn
176. two
177. vomit
178. walk
179. warm
180. wash
181. water
182. we
183. wet
184. what
185. when
186. where
187. white
188. who
189. wide
190. wife
191. wind
192. wing
193. wipe
194. with
195. woman
196. woods
197. worm
198. ye
199. year
200. yellow

which I think would be most likely to indicate relationship, when such existed" (1891: 334). Of the 21 items of meaning on the list, 17 are found also on the revised glottochronologic test list (*man, woman, sun, moon, fire, water, head, eye, ear, mouth, nose, tongue, teeth* [*tooth*], *hand, foot, one, two*), three are on the earlier list (*three, four, five*), and only one item is on neither (*house*). In demonstrating California Penutian, Dixon and Kroeber (1919) used a "Basic List" of 128 different items of meaning. (There are 171 numbered items, but each different set of correspondences for an item of meaning is given a different number.) A number of items (34) represent flora, fauna, and material culture appropriate to the area, but, of the total list, there are

100-Item Test List

Diagnostic Items

1. I
2. thou
3. we
4. this
5. that
6. who
7. what
8. not
9. all
10. many
11. one
12. two
13. big
14. long
15. small
16. woman
17. man
18. person
19. fish
20. bird
21. dog
22. louse
23. tree
24. seed
25. leaf
26. root
27. bark
28. skin
29. flesh
30. blood
31. bone
32. grease
33. egg
34. horn
35. tail
36. feather
37. hair
38. head
39. ear
40. eye
41. nose
42. mouth
43. tooth
44. tongue
45. claw
46. foot
47. knee
48. hand
49. belly
50. neck
51. breasts
52. heart
53. liver
54. drink
55. eat
56. bite
57. see
58. hear
59. know
60. sleep
61. die
62. kill
63. swim
64. fly
65. walk
66. come
67. lie
68. sit
69. stand
70. give
71. say
72. sun
73. moon
74. star
75. water
76. rain
77. stone
78. sand
79. earth
80. cloud
81. smoke
82. fire
83. ash
84. burn
85. path
86. moun-tain
87. red
88. green
89. yellow
90. white
91. black
92. night
93. hot
94. cold
95. full
96. new
97. good
98. round
99. dry
100. name

Supplementary Items

101. ye
102. he
103. they
104. how
105. when
106. where
107. here
108. there
109. other
110. three
111. four
112. five
113. few
114. sky
115. day
116. fog
117. wind
118. flow
119. sea
120. lake
121. rain
122. wet
123. wash
124. snake
125. worm
126. back
127. leg
128. arm
129. wing
130. lip
131. fur
132. navel
133. guts
134. saliva
135. milk
136. fruit
137. flower
138. grass
139. with
140. in
141. at
142. if
143. mother
144. father
145. hus-band
146. wife
147. salt
148. ice
149. snow
150. freeze
151. child
152. dark
153. cut
154. wide
155. narrow
156. far
157. near
158. thick
159. thin
160. short
161. heavy
162. dull
163. sharp
164. dirty
165. bad
166. rotten
167. smooth
168. straight
169. correct
170. left
171. right
172. old
173. rub
174. pull
175. push
176. throw
177. hit
178. split
179. pierce
180. dig
181. tie
182. sew
183. fall
184. swell
185. think
186. sing
187. smell
188. puke
189. suck
190. blow
191. fear
192. squeeze
193. hold
194. down
195. up
196. ripe
197. dust
198. alive
199. rope
200. year

64 items that are found also on the revised glottochronologic list, and 12 others that are found on the earlier list. These 76 out of the 94 general items on the Dixon and Kroeber "Basic List" constitute an identity of 81%. And it is worth noting that all but two of the meanings cited from Meillet (in the preceding section) are on the basic vocabulary test lists used in glottochronology. In sum, a variety of experiences with items of meaning that have been useful in field work and in historical study because of their universality and persistence seems to have anticipated the empirical experience of glottochronologic work.

We should review here how the items on the glottochronologic test list have changed in the course of revision. The pioneer Salishan study (Swadesh 1950) used 165 items (and a provisional retention rate). Lees (1953) based his work on 13 control cases in which 215 items were used, but Swadesh's first general paper on the method (1952) had already shortened this to 200, although retaining the approximately 81% average retention rate calculated by Lees. This 200-item list was used for the *Amerindian Non-Cultural Vocabularies* as first distributed (Swadesh 1954–). This revision had dropped 16 items (the 15 listed in Swadesh 1952: 457, plus the unlisted "*berry [or fruit]*"), had changed *speak* to *say*, and had added *heavy*. The radically revised 1955 list has 100 preferred or diagnostic items for which a new retention rate of approximately 86% is determined. It has 92 items from the list used by Lees, plus eight additions (*say, moon, round, full, knee, claw, horn, breast*). A supplementary list of 100 items is drawn largely from the remainder of the 200-item list. Both the diagnostic and supplementary lists are used in the revised editions of the *Amerindian Non-Cultural Vocabularies* (Swadesh [March] 1955–); the diagnostic list is to be found also in Swadesh (1955*b*), where 92 items are starred in Table II (pp. 133–37) and the 8 additions are listed elsewhere (p. 124).

I know no adequate or direct study of the *frequency* of the items on the glottochronologic test lists. Of coincidental interest because of its size is a "minimum semantic alphabet" devised by Rose (1936). It contains all those words which were given as among the 500 commonest English words in every one of six different sources. The 200 words of this "common core" were also confirmed as among the first thousand in various frequency lists of French, German, Spanish, and Italian. Rose's 200 words include only 46 (23%) to be found on the 200-item glottochronologic list, and only 26% of the revised, 100-item list. (Rose's list contains no colors, flora, fauna, or natural phenomena, and but two body parts, all of which figure in the glottochronologic lists, especially body parts.) Very likely this is too narrow a perspective on frequency for our purpose, but it demonstrates that the test lists are not based on a conjunction of the criteria of frequency and universality.

For frequency in a broader view, where individual meanings are noted, Eaton (1940) and Lorge (1949) show about the same proportions of test-list items in the 500 most common English words. (Lorge has 570 words, comprising the first 500 in both Thorndike [1931] and Thorndike and Lorge [1944].) Of the 200-item list, Eaton has 56%, Lorge 58.5%, in the first 500. Of the 100-item list, Eaton has 64%, Lorge 63%, in the first 500. More broadly yet, Eaton has 79.5% of the 200-item list in the first English thousand, 84% of the 100-item list. (And for the first Spanish thousand, 73% of the 100-item list.)

Turning to purely spoken material and the early learning of children, Horn (1925) shows 77% of the 200-item list in the "commonest words in the spoken vocabulary up to and including six years of age" (total 1,116), and 87% of the 100-item list. Rinsland's "basic vocabulary" of elementary school children (1945), which draws on a wide sample of the U.S. shows less: 65.5% of the 200-item list among the thousand most frequent for the first grade, 70% of the 100-item list. More broadly still, for the 200-item list in English, Eaton shows 90.5% in the 2,000 most frequent English (written) words, Rinsland 82.5% (for spoken first-grade material); for the 100-item list, Eaton shows 92% for English and 89% for Spanish, and Rinsland 86% for English, in the first 2,000.[5]

Firm conclusions cannot be drawn, because none of the data was obtained with problems of glottochronology in mind. Two points of interest do emerge. Far from all test-list items are among the most frequent, but a strong majority is; and the proportion among the most frequent is consistently greater for the 100-item list. (See Table I.) This indicates a strong positive correlation between frequency and the basic vocabulary tested by the glottochronologic lists, a correlation that increases with the 100-item list, which was revised for "definite methodological needs" (Swadesh 1955*b*: 127) having nothing to do with frequency.

TABLE 1

PERCENTAGE OF TEST VOCABULARY IN FOUR LISTS OF COMMONEST ENGLISH WORDS

Source	*200-item list*			*100-item list*		
	First 500	First 1,000	First 2,000	First 500	First 1,000	First 2,000
Lorge *	58.5%			63%		
Eaton	56 %	79.5%	90.5%	64%	84%	92%
Rinsland	45.5%	65.5%	82.5%	51%	70%	86%
Horn †		77 %			87%	

* 570 words † 1,116 words

Still, it is clear that commonness has been assumed rather than investigated, so far as the concept of basic vocabulary in historical linguistics, and the glottochronologic test lists, are concerned. And it is clear that the attribute of frequency by itself would yield quite different lists for different languages. The development of the test lists so far has given priority to the attribute of *universality*, for obvious practical reasons. The goal has thus been for a test list to constitute a *least common denominator* of the basic vocabulary sector in the languages of the world. With what success?

In a critique of the assumption of universality, Hoijer discusses the problem of obtaining a single, simple, common, and clear equivalent for each item of meaning on the test list. He writes (1956*a*: 53):

> The brief history of the test list which is summarized above makes clear (1) that it was originally set up largely in terms of European languages and cultures . . . and (2) that it has been refined and improved in quality mainly by its application to non-European languages and cultures. The original list, it turns out, included many items (more than fifty per cent) which do not meet the criteria set forth, but this fact was not discovered until attempts had been made to translate the list into a number of divergent languages. It seems likely, moreover, that the difficulties noted to date will occur again, as the list is further applied to still other

languages. There is, in short, nothing in lexicostatistic theory which enables us, once and for all, to establish a firm test list, translatable, without the difficulties enumerated, into any language.

The central contribution of Hoijer's critique is a demonstration from Navaho and Athapaskan that "each language has not only its own peculiar structure but its own unique set of semantic patterns as well" (1956*a*: 59). Quite clearly, to the attributes of commonness, universality, and historical persistence cannot be added identity of semantic patterning in the languages of the world. The basic sector does not differ from the rest of the vocabulary in this regard (cf. Velten 1947).

Hockett has remarked, "The number of meanings for which a human language *must* have words seems amazingly small" (1958: 530). Within the sphere of such meanings, it is clear that Hjelmslev (1958: 664) and others are correct in rejecting any assumption of a set of pre-established universal ideas or meanings for which different languages merely have different tags. At this point, glottochronology encounters and confirms Sapir's dictum that, "The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached" (Sapir 1929: 209).

That practical difficulties may always appear in the application of the test list has been recognized from the start: "Of course, it would be impossible to devise a list which works perfectly for all languages, and it must be expected that difficult questions will sometimes arise" (Swadesh 1952: 457; cf. Brinton 1891: 334). At first it was hoped that problematic items could simply be dropped in a particular application, and that there would be too few to affect results. It is now clear that this is not feasible. But it is possible to expect that, in filling out the list for a language, a fair number of items will be problematic, and to regard this as a limitation to be met by uniform rules of procedure for choosing among alternatives, or for substituting other items. Some proposals in this regard are included in the general discussion of problems of application, below. In this, as in other such matters, to recognize the difficulty is half a contribution; the other half is to help solve it.

In sum, the criterion of universality is far from easily met, but it seems possible to meet it approximately in a way that permits the method to be used. The most explicit critic on this score, Hoijer, has used the method for some of the most valuable results it has obtained (Hoijer 1956*b*; see also Hymes 1957*b*). Hattori remarks (1953: 74):

> Although it is possible to comment critically on the method at several points, its effectiveness in general should be acknowledged. For instance, there are no 'concepts and experiences common to all human groups' (Salish Internal Relationships, 157), because the dispositions of the speakers of different languages are often different even to 'the same things,' that is, the 'sememes' (as the author calls them) of synonymous words of any two languages are often not identical. Thus, Japanese /me/ 'eye' denotes the eye which opens and closes rather than the eye-ball, while (Chakhar) Mongol /nuda/ which is the only word for 'eye' denotes the latter rather than the former. Nevertheless, we can find words which refer to things and events common to all human groups, and thus we can generally fill Swadesh's questionnaire.

Turning to the attribute of persistence, we have noted above the long-held view that some types of vocabulary are less likely to be replaced than others, although for any specific semantic group, such as kin terms, or numerals, exceptional cases can be found. When it comes to a specific test list of meanings, two types of questions are of major importance. Is the list as a whole relatively resistant to the distorting effects of such factors as tabu and borrowing? Are the individual items sufficiently homogeneous with regard to historical persistence?

The items of the test list are not exempt from the ordinary forces of cultural change. Indeed, it may be that a word or meaning is likely to become the focus of specialized meaning in direct proportion to its basic or fundamental character. What do the religions and rituals of the world reveal to us if not exploitation and elaboration of symbolism about parts of the body such as hand and eye, natural phenomena such as the sun, moon, wind, and fire, numerals such as one, two, and three, and so on? Thompson's *Motif-Index of Folk Literature* would be a gutted set of books if all references to the items on the glottochronologic test list were removed. The problem has been recognized by the developers of glottochronology: "Any word whatsoever may take on cultural implications. We can only avoid those which are potentially culture terms in a relatively high degree" (Swadesh 1955*b*: 125). In point of fact, the danger is that a term dependent on a particular feature of a culture will be subject to the same flux as the culture trait itself. Use as a fundamental source of metaphor and symbolism, and in a variety of contexts, may constitute a cultural entrenchment, and hence a source of stability for items of basic vocabulary. It becomes an empirical matter to discover the nature of change in the sector of vocabulary tapped by the list.

With regard to word tabu, Elmendorf (1951) has pointed out that the greater internal diversity of Coast Salish vis-à-vis Interior Salish, as calculated by glottochronology, coincides with an area of word tabu. He suggests that this practice, by accelerating lexical change, might account for the Coast Salish diversity rather than greater lapse of time. A check by Swadesh showed that Salishan languages with word tabu practices have changed no faster in basic vocabulary than those which lack them. As Swadesh states: "We cannot merely take it for granted that any given cultural influence must skew the rate of change in fundamental vocabulary. Instead it is necessary to test carefully every hypothesis that may be suggested" (1952: 459).

Marshall (1956) discusses a striking tabu effect in Tahitian. According to the mid-nineteenth-century account of Horatio Hale, words resembling the name of the king used to be tabued during his reign, but restored to use at his death. In the initial period of White contact, missionary translations into Tahitian fixed in usage some otherwise temporary replacements. This intervention is said to have altered the retention rate for that period, but the average retention rate over the whole period from proto-Polynesian times to the present seems within the normal range. Bergsland has argued

that word tabu for body-part terms has distorted the glottochronologic results for East Greenlandic and West Greenlandic (1958: 655–56). In the 200-item list the common fraction for the two is 78.7, but it is noticeably smaller in the shorter 100-item list (71.9); since body-part terms form a fifth of the shorter list, but only about a tenth of the longer list, their rapid replacement due to word tabu would have greater effect on results obtained by the former; if the two dialects separated 500 years ago, the retention rate per millenium for the shorter list would be about 72%, which is somewhat lower by two or three points than the results used by Lees to calculate the average retention rate. However, the inclusion of the case would not particularly affect the average. More important, no evidence is presented that the separation of East and West Greenlandic should be put at just 500 years ago, nor is the practice of word tabu in the different Eskimo areas analyzed. It is important to note the effect of a particular semantic area, such as body parts, but that tabu has had a distorting effect is not shown. (The Eskimo case will be discussed again below, under "Application.")

The influence of borrowing on the test list has been discussed by Gudschinsky (1955), Sjoberg and Sjoberg (1956), and Swadesh (1956*b*); its importance has been asserted by Hjelmslev (1958). The problem is that a high rate of borrowing might distort the retention rate in the particular case, and thus lead to mis-estimating time depth. For a given period of time, borrowings might reduce the fraction of items common to related languages, and thus induce overestimation of time depth; or, borrowings between related languages or dialects might increase the common fraction of apparent retentions, and thus induce underestimation of the real time depth.

Swadesh reports that "borrowing appears as a serious factor in less than one per cent of the cases thus far examined" (1956*b*: 305). Examining two hundred New World test vocabularies, he finds that nine-tenths have no borrowed elements from Indo-European languages, several have one or two per cent, and only one (Mecayapan Nahuatl) has a high rate. Gross amount of borrowing and rate of borrowing are an interesting lexicostatistic study in themselves, but the critical thing for glottochronology is the effect on the retention rate per millenium.

Now, it is necessary to stress that glottochronology does not deny the role of factors such as tabu and borrowing, and it is no critique of the method to point out that they occur. The method does stress a certain rate of *retention*, but it also, necessarily, stresses a complementary rate of *replacement*. Something must cause the replacements—borrowing, word tabu, or any other factor that may bring shift of meaning or loss of a word. No matter how high the rate for any one factor, if the cumulative sum of all such effects remains at about the postulated level (14% for the revised 100-item list), no difficulty for the method ensues.

The problem cases presented so far involve conquest (Dominica), a prestige literary language (Sanskrit and Dravidian Kannada), and more or less closely similar dialects (Mazateco; Dano-Norse and English). Clearly such situations do not necessitate a high rate of borrowing. The Norman conquest has had almost no effect on the test list equivalents for English, and it is the same with Latin, the literary language of England for some centuries. The New World results mentioned above show a similar picture for the effect of the prestige and literary languages of Indo-European-speaking conquerors on American Indian languages. (The question of dialect borrowing will be taken up under the topic of the use of lexicostatistics for sub-grouping.)

One case has been analyzed in detail, that of Kannada in India. The high rate of borrowing from Sanskrit, when added to other factors, results in a replacement rate of 18% over-all, and thus a retention rate of 82% per millenium (Swadesh 1956*b*: 302, 304). This is a deviation of 4% from the 86% for the 100-item test list, but 4% is less than the deviation of some of the original 13 control cases from the average retention rate established for the 200-item list, and 4% is just at the border of the standard deviation from the 86% retention rate, as determined for seven control cases and six control cases (Swadesh 1955*b*: 130). Despite an exceptionally high rate of borrowing per millenium, then, Dravidian Kannada actually exemplifies the tendency for basic vocabulary to change at a regular rate. Indeed, it might be added as a control case.

If the percentage of cases with high rates of borrowing remains so small (Dominica so far outstanding), and if other cases do not exceed the known deviation of Kannada for borrowing in basic vocabulary, this source of distortion will remain minor, one the potential presence of which can perhaps be detected from known historical facts. That basic vocabulary generally is less subject to borrowing seems borne out by every investigation and is commonly assumed. In Kannada the non-basic vocabulary has borrowed from Sanskrit at a rate perhaps double that of the basic sector. Greenberg refers to "the a priori expectation and the historical documentation of the thesis that borrowing in culture words is far more frequent than in fundamental vocabulary" (Greenberg 1957: 39). It is striking to use the test list for eliciting and have a Turkish speaker exclaim at getting at the "real" Turkish words, i.e., not the overlay of Arabic and Persian loans. But, as with other aspects of the subject, further detailed studies are devoutly to be wished for. It remains to stress again that borrowing in any one part of basic vocabulary must be weighed against the whole, and that rate of borrowing at any one period must be weighed against the average rate of change per millenium.

Three comparisons not involving particular causes of change have been made between test lists and other areas of vocabulary. Lees (1953) examined all English entries in Buck's *Dictionary of Selected Synonyms in the Principal Indo-European Languages* for cognates retained from Old English. Many of the morphemes were from non-basic areas. The retention rate was nearly 30% lower than for the 200-item list. The retention rate for all English items from the chapter on body parts and functions by itself was still 13% lower than that for the list. Pierce compared two almost mutually unintelligible Siouan languages, Crow and Hidatsa. The percentage of mutual intelligibility for the 200-item list was 31%, in contrast to 22% for a second list of 50 nouns

chosen by Pierce. This result "is certainly indicative that the words in the Swadesh list have been obscured less by language change within the languages than have some other sections of the vocabulary" (Pierce 1954: 136). Olmsted compared Lucumi, an African-derived language of Cuba, with Yoruba of Nigeria. He found that the basic-root-morphemes appear to have been replaced about 10% faster than the "cultural" vocabulary. Olmsted points out the possible interpretation that "the social conditions incident to the formation of the Lucumi-speaking group were so special as to produce a language unclassifiable either as of genetic descent or as pidginized" (1957: 842), and that such a case might not be expected to conform to the assumptions underlying glottochronology. The proper classification and interpretation of such cases is indeed in dispute. Their potential contribution to our knowledge of linguistic change is very great, but at present they should be considered a special category.

We would like much more evidence, but what clear evidence we have supports the view that basic vocabulary, including that on the glottochronologic test list, changes less rapidly than other areas of vocabulary.

It is not to be expected that the test list items will have identical persistence, but their homogeneity in this respect has a practical import. Lees notes (1953: 127):

> If there are classes of basic-root-morphemes which decay at different rates, the assumption of a temporally constant k (retention rate) for our word list (which presumably contains members of several such classes) is clearly false. Through a given time period, as the resistant classes survive the less resistant, their concentration in the sample will increase, and the value of k will rise.

Gleason☆ puts the theoretical point saliently with a hypothetical illustration:

> The rate "constants" which we have are in fact *average* rates for the various items in the list. Averages cannot be used in an exponential equation. Put another way: With each succeeding millenium a smaller proportion of the retained items are of the subgroup with the lowest retention rate. Therefore the average retention rate rises constantly in time. The effect of this may be seen from an example: Assume a list exactly one half of which has a .90 retention rate, and one half a .70 retention rate. The average is therefore .80. With a divergence of five millenia, we would expect retention of .184 of the list vocabulary (equals $(.90^{10} + .70^{10})/2$). Calculating time depth from this figure on the assumption of an .80 retention rate, we get 3.8 millenia. We have missed by 1.2 millenia. At deeper time depths, it gets very much worse. And if there is greater variation in actual retention rates, the results will be worse.

Swadesh discovered variation in retention rate to be in fact the case in the items used to determine the rate constant (1955*b*). This was after he had reconstituted a 100-item list on theoretical and empirical grounds, and then compared the persistence scores in control cases for its items with those for the 200 items of the earlier list. The distribution of individual persistence scores for the 200-item list is bi-modal, being concentrated within the areas of retention for 51–60% and 81–100%, respectively, of the total number of decades covered by the control cases. The new list has a smaller range and a single area of concentration (81–100%) (although 8 additions are not scored).

Such a result is reasonably satisfactory. Differences among items can still result in underestimation of divergence times, but to a lesser extent and at greater time depths. The result, however, is based on only a few reliable case-histories and needs investigation for a larger number.

Two tests of homogeneity for the original list have given positive results. Lees split the German/English entries into two parts, and found the fraction of shared items for the two halves to differ only by .010 (Lees 1953: 124). Hirsch, having broken his test list into four sub-lists of 184, 144, 108, and 100 items, respectively, subjected those Eskimo dialects for which it was possible to test on all four. "The results of this test showed very little variation in the percentage of cognates among the various sublists. . . . Therefore, the total list can be considered homogeneous and the results of the word counts of the sublists are comparable" (Hirsch 1954: 825). (Gudschinsky obtained an indication of homogeneity for the 200-item list with a larger sampling of Mazatec basic vocabulary [1955: 139, n. 6].)

In view of Swadesh's demonstration of heterogeneity, why should these tests show homogeneity? One answer would be that the effect of the heterogeneity in the 200-item list does not appear within one and two millenia, the range within which these cases fall, but only over longer periods. This would indicate that the improved 100-item list would show no undue effects from heterogeneity for appreciably great time depths. Another possibility is that the heterogeneity is present not so much with respect to individual items, as to semantic groups, but that it is randomly distributed across them in a list that is arbitrarily cut. (The 200-item list is ordered alphabetically.) David H. Kelley (personal communication) has found that when cognates on the list, for a group of Uto-Aztecan languages, are arranged by semantic classes, there is a notable disproportion between verbs of motion, adjectives (terms of size and description), and grammatical features (location and a number of groups on the list are morphologically expressed), on the one hand, and the rest of the list (body parts, natural phenomena, kinship, numbers, colors, etc.), on the other. The proportion of cognates in the first set is low, the proportion in the second set high. According to Table II in Swadesh (1955*b*), Athenian Greek shows persistence of none of the five color terms used on both lists, while Chinese shows persistence of them all; Athenian shows persistence of none of the four animal terms on the 100-item list, while Chinese shows persistence of all but one; Athenian shows persistence of both location terms on the 100-item list, while Chinese shows persistence of neither. With regard to color terms, there seems indeed to be a sorting out by family within Indo-European: English, German, and Swedish show persistence of all five; Athenian and Cypriotic Greek show persistence or retention of none; and the Romance languages in the sample—French, Rumanian, Spanish, Catalan, Italian, and Portuguese—show intermediate scores from three to one. Comparison of the persistence of color terms in Indo-European and Romance, so far as shown in Swadesh's Table II, with Hoijer's Athapaskan data (Hoijer 1956*b*) shows consistent differences. There are five color terms and eleven Indo-European cases, and the total number of

persistences or retentions is 28 out of a possible 55; the number of Athapaskan cases varies between 13 and 15, and the total number of retentions is 51 out of a possible 68. For Romance languages (7 cases in Swadesh's Table II), the corresponding figure is 13 out of 35. For the color terms, then, Romance languages show persistence or retention about one-third of the time, Indo-European generally just about half the time, and Athapaskan just three-fourths of the time.

Such findings are suggestive enough to argue for the importance of carefully analyzing the test list in terms of semantic groupings. Swadesh (1955*b*: 132, Table I) makes a provisional sorting by semantic groups for the 200-item list. Such analysis for the 100-item list in many language families is necessary to understand fully the degree of homogeneity the list possesses. It also may shed light on characteristic differences among language families with regard to retention in semantic groups, and it may have important consequences for the adaptation of the test list to particular cases.

With regard to semantic groups, it seems unwise to have the present list so weighted for body parts and substances (group [p], Table I, in Swadesh 1955*b*: 132). These comprise a good fourth (26 items) of the list; all but three of those on the original 200-item list are retained. (Body parts alone comprise a fifth of the 100-item list.) We have mentioned the problem of East and West Greenlandic, but, beyond this, the general possibility of differential retention of semantic groups advises a different reconstitution of the list. As we have said, about half the original list is not used in the new diagnostic list. Good reasons are given for not using some of the items: tendency to be sound-imitative, to duplicate other items, to be interlingually ambiguous and unstable, and so on. There remain many items for which no explicit reason is given. Of this remainder, a good number match the items of group (p) (body parts and substances), so far as range of persistence scores is concerned. Some of this number, such as *hold, live, other, sky, tie, wash, wide, wind, year,* could replace group (p) items, and so give the list better spread across semantic groups. Also, *three* might be retained; it is rejected because a few languages lack a simple term for it, but none of the test items has a simple equivalent in all languages anyway.[6] For that matter, the exclusion of useful but structure-bound items, such as *and, at, in,* seems optional. Since most languages are not of the "isolating" type, and since, as Sapir among others has discussed (1921: 109–13), a particular meaning may find itself in a variety of grammatical statuses, morphological control of a language is prerequisite to the use of glottochronology in any case.

Note that the basic sector in a given language is probably larger than the approximate least common denominator that a test list samples. Certainly this seems true for basic vocabulary as defined for a language by the attribute of frequency alone. Any set of the most frequent items in a language that included a sizable portion of test-list items would include many other items as well. Empirically, Gudschinsky has added 50 basic items to the 200-item list without affecting the classification of Mazatec dialects into groups (1955: 139, n. 6). Within Indo-European, all numerals from *one* to *ten* are uniformly persistent (as also in Coptic and Chinese), although the revised diagnostic list could retain only *one* and *two* for general use. This scope of basic vocabulary in a language has practical implications. To take a mock example, if the equivalent for *one* could not easily be found in a glottochronologic study of Indo-European, any other numeral up to *ten* could be substituted with equivalent results, so far as persistence within that family goes. But knowledge of the behavior of semantic groups is needed to take advantage of such possibilities (and this implies control of reconstructed vocabulary through the comparative method).

Theoretically, then, the basic sector of vocabulary is as much a matter of groups, defined by frequency, meaning, or persistence, as it is of particular items. In different languages, different items may equally well satisfy one or more of these criteria. Thus, different but equally valid lists for each of different language families are theoretically possible.

Although we can describe the basic sector in general by its attributes, we cannot expect this sector to be identical with—fully defined by—a single list. Nor can we expect each item of any finite list to be basic in every respect. Within a language, the criteria of frequency, universality of semantic reference, and persistence, define sets that intersect but are not identical.

Thus, we can expect only substantial overlap for the items at the basic pole, when different criteria are applied. We can expect only substantial overlap when different languages are compared.

The problem of a glottochronologic test list is to sample the basic sector in each language in as useful and consistent a way as possible.[7] The criterion of universality of reference, whether of groups or items, must loom large. High persistence is desirable for long-range work, but reliability or predictability of persistence is most important. (A completely persistent list would provide no dating at all; a list of low persistence would index recent divergence times more exactly.) Frequency, it seems, must take something of a back seat, as it has so far, but, for theoretical understanding of basic vocabulary—for explanation of how there can be such a thing as basic vocabulary—it must be investigated.

Gleason☆ has made a more general proposal with regard to the assumptions underlying the selection of test-list items. He suggests the premises:

> (1) Every lexical item at every given time has a certain probability of change.
>
> (2) This probability of change is variable, and is influenced by both linguistic and non-linguistic factors.
>
> (3) There exist certain sets of largely independent vocabulary items in which the probability of change within the group is large relative to the variability of that probability of change.

In Gleason's view,

> The first two premises are implicit in glottochronology as hitherto stated, and are in no sense new. The third is the important one. It does not assume any characteristic of the vocabulary items concerned other than a relative stability in rate of change. Moreover, it makes no assumption about the absolute amount of that probability of change. It might be either very high or very low. Parenthetically, it would be

ideal if several such sets could be found, one with high rate for measuring shallow depths, and one with low rate for measuring deep depths. [This suggestion has been put forward before by Swadesh.] This may be too much to hope for, and we should be pleased if we can find at least one useable set.

Premise (3) is said to "have the advantage of being more readily testable than that which it replaces" (i.e., the early concept of "non-cultural" vocabulary). "The testing is complex and lengthy, but feasible." As for the distinction between basic and non-basic vocabulary, Gleason states that it "retains some usefulness, but cannot be given any precision, and hence should be avoided, in lexicostatistic contexts where we are aiming at precision, if possible."

In my view, Gleason's three premises are valuable, clarify what is left vague in my allusion to reliability or predictability of persistence, and deserve full testing. At the same time, I feel that a distinction which retains usefulness need not be avoided. Discussion of the validity of the test list based on the concept of basic vocabulary has shown some exactness by convergence in our conceptions and the items we choose. A fair amount of empirical information has been acquired about the properties of vocabulary items in regard to the basic attributes, and a good deal more can and should be accumulated. It has value both within lexicostatistics and elsewhere. Moreover, since the concept of basic vocabulary involves empirical attributes, it does not interfere with precision that may be attainable in estimating probability of change and its variability. Indeed, the attribute of frequency is closely related to the attribute of probability of change stressed by Gleason. (See Dyen [2].☆) Finally, as later discussion shows, I regard hypothesis and proof of genetic relationship as a lexicostatistic context in which basic vocabulary is quite important.

Gleason☆ also suggests that it may not be feasible or fair to comment on the frequency of the Swadesh glosses in, e.g., Lorge, because the glosses were picked not necessarily as the commonest word for a given meaning, but as the one judged to be the least ambiguous gloss; the list is not, then, a word list for English, but remains primarily a gloss list. Note, however, that sources such as Eaton include languages other than English, and that insofar as Lorge, Eaton, and others succeed in their goal of providing counts of "semantic frequency," they are appropriate to a gloss list. Thus discussion of frequency is not specific to English forms.

In general, I consider the notion of frequency to be essential to an explication of the concept of basic vocabulary. It enters explicitly into the method of glottochronology itself, since the instructions for obtaining equivalents to the gloss list specify choice of the commonest. The notion of frequency is also interdependent with the other attributes of basic vocabulary. It is involved in that of universality, since, as Dyen [2]☆ points out, universality implies a minimal frequency in every language. As noted above, rate or probability of change also entails consideration of frequency. In this regard, Dyen's [2]☆ original analysis of aspects of frequency, including the remarkable hypothesis of an average culture-free frequency for some meanings—one not obviously supported by existing information—deserves empirical testing. Positive data from a variety of cultures would be of great interest in several fields. Anthropologists should not neglect the implications of word frequency, which may perhaps be tapped rather efficiently by word association.

To sum up, there are indications that the present diagnostic list of 100 items is useful, consistent, and valid in large measure. There are also indications that the best possible general list has not yet been achieved, and that the construction and testing of special lists should be undertaken. Hattori [2]☆ has conducted a study of basic vocabulary in order to enlarge the test list. A list of 457 items was provisionally published in August, 1957. This study is important, partly because it draws on Japanese research, which has not been taken account of in European work. Thus it provides a contrastive perspective on questions of frequency, universality, and translatability.

A beginning has been made in knowledge of the basic vocabulary as sampled by the glottochronologic test lists, but only a beginning. Further knowledge will affect glottochronology, and shed light on other aspects of cultural change.

CONTROL CASES

In the control cases, there is the general difficulty of dating the earlier stage exactly; inexactness increases the range of error in the calculation of the retention rate. In some control cases, there is a question of the actual continuity between the earlier and later stages. In six or seven hundred years, science may begin to enjoy a multitude of good control cases from all around the globe, if only enough languages survive. In the meantime, we have to make do with the handful of languages that were written 1,000 years or more ago. Swadesh has based his recalculation of a retention rate (for the revised 100-item list) on three exceptionless cases (although the three rescored by Arndt (1955) might have been included). This certainly does nothing to change his observation that "lexicostatistics has thus far operated, as it were, on a shoe-string of basic research" (Swadesh 1955*b*: 122).

One impressive addition to the shoe-string has been made by Hattori (1953), who has studied four dialects of modern Japanese in relation to Old Japanese. Since Old Japanese can be dated in the eighth century, an independent test of the retention rate was possible. Hattori's results, using the 200-item list, fit very closely the average retention rate calculated by Lees as 80.5% plus or minus 1.8%. He found retention rates per millenium from Old Japanese of 78.4% for Kyoto, 79.0% for Kameyama, and 80.4% for Tokyo. (The relation of the fourth dialect, Shuri of Okinawa, is a special problem.) Another confirmation, although qualified by the short period involved, is Taylor's finding for two Island Carib dialects (1952: 229):

> The percentage of correspondences in the above list (94%) agrees with the unit retention rate of 80% (per 1000 years) worked out for this language by Swadesh and myself, if it is presumed that the two dialects were identical as to basic vocabulary at the time of the Black Caribs' deportation in 1797, and that the latter's dialect has diverged during 150 years, that of the Dominicans for little more than 100 years (because it became extinct).

It was noted earlier that Dravidian Kannada, rather than constituting an exception, also confirmed the average retention rate. A recent note on three Arabic dialects has suggested that there may be a different rate-constant for each family of languages, and, particularly, that the retention rate for Arabic is higher than average (Samarrai 1959). Once an error of procedure is corrected, however, these results also fit. Samarrai uses 211 of the 215 items employed by Lees, but, in the case of 22 diverse items for which Classical Arabic apparently has synonymous equivalents, he counts the group as a whole as retained. This gives each item a double chance of being retained (in one case three chances), and each dialect in fact retains one or the other equivalent for almost every item in the group. The original procedure proposed by Swadesh and Lees was to drop problematic items. This procedure would give a somewhat lower retention rate for Egyptian, Iraqi, and Jordanian, but still a high one. It is clear, however, that the best alternative in such cases is to choose one equivalent for each item by some explicit criterion—if necessary, by chance. If either the first listed equivalent for each item, or the second, is arbitrarily chosen from Samarrai's list, the result is to bring the retention rates for each of the three Arabic dialects toward the higher values obtained by Lees. Indeed, if added to his values, a recalculation of the mean rate-constant gives .8155 as compared to Lees's .8048, which hardly disturbs Lees's statement that, "We take this to mean that on the average about 81% of the basic root-morphemes of a language will survive after 1000 years" (1953: 119). (See Hymes 1959*b*.)

Satterthwaite (1960) has completed a well-documented control-case study of Meccan Arabic, using the Qoran as source for the earlier stage, and contemporary Meccan informants for the later stage. He was able to determine Qoranic equivalents for 88 items of the 100-item list. Satterthwaite remarks that the daily public recitation of the Qoran in mosques and schools would lead one to expect that its vocabulary would be especially stable, and that such stability would result in a retention rate higher than usual. However, the retention rate for Meccan Arabic is found to be both quite close to the 86% average rate found for the 100-item list by Swadesh, and also somewhat less than it. Although Satterthwaite's three informants gave different equivalents for some items, all three of the resulting lists showed identical retention rates of 82.3% (68 out of 88 items). A composite Meccan Arabic list, made by selecting for each item the form on which two or all three informants agreed, showed a retention rate of 83.1% (68 out of 87 items—for only one item did each give a different response). However, it is uncertain that the semantically appropriate Qoranic equivalents were chosen in every case; see below, under "Adapting the List."

The only serious exceptions known, then, are those adduced by Bergsland (1958). He reports that the retention rates between southwestern Norwegian and Icelandic, between Modern Georgian and Modern Mingrelian, and between Modern Georgian and Old Georgian of the seventh to the tenth centuries, are gravely higher than those otherwise reported. The first two cases seem to involve the question of underestimation (discussed later with regard to applications, especially Romance) from comparing contemporary languages, rather than a direct check on the retention rate in a single line between two stages of the same language. It is to be hoped that the supporting data will be published, since it is clear from the above Arabic example, and from other examples to be discussed, that investigators may differ in their application of the method in ways that seriously affect results.

The major criticism to be made with regard to the control cases so far reported concerns their representativeness. All but two of Lees's cases are Indo-European (English, Spanish, French, German, Coptic, Athenian, Cypriote, Chinese, Swedish, Italian, Portuguese, Rumanian, Catalan), and the three cases that Swadesh uses for the retention rate for the 100-item list (because they give the narrowest standard deviation) are all Indo-European (Rumanian, French, Athenian) and, indeed, two of the same sub-family, Romance. For this reason, the cases of Japanese, Kannada, and Arabic carry special weight. Now, it may be that all the control cases available in the world today would not add up to a sample truly representative of the world's languages. Anthropologists such as Jacobs (1954) and Driver (personal communication) have expressed grave doubt that basic vocabulary should prove an exception to the rule that generalizations based on Old World data alone do not suffice for the cultures and peoples of the New World. As part of language, basic vocabulary is part of culture, and hence subject to any differences in the processes of culture change that exist between one area and another. Although the trend of agreement in cases outside Europe and Indo-European is encouraging, one can hardly be content with less than full results from all possible control cases. If from different language families and parts of the world these continue to agree, the more sanguine of us probably will waive the question of statistical representativeness, and of the variable that is insurmountable in the nature of the case: does the fact of having been written for about a millenium, necessary for control cases, itself make a difference? To repeat, the degree of agreement among reported control cases is exciting, but we do not yet have the major research that can definitely establish the facts of the matter. It is of course true that even if control cases should differ markedly in rate of change, an average rate of change and its standard deviation would still permit estimates of time depth (Driver [4]☆). But, if the deviation is very great, the usefulness of the estimate for historical anthropology will be little or nil. Therefore it is to be hoped that further studies will confirm the degree of agreement so far found.

RETENTION RATE

The validity of the retention rate is dependent upon the factors discussed above under "Control Cases," but, in addition, one major criticism must be made. Neither the 200-item list nor the 100-item list has been tested for rate of retention in the exact form in which it is used. The 200-item list drops 16 items from the Lees list for which the 81% retention rate was determined, and adds one the persistence of which is untested or at

least unpublished. The 100-item list adds eight items the persistence of which is not included in the calculation of retention rate for the list.

A second limitation on the validity of any rate of retention is the time depth of the control cases. In the cases studied by Lees, as in the others discussed, the time depth ranges from one millenium to slightly over two, and additional control cases are likely to fall within this same range for a long time to come. In consequence, it is not possible to verify in principle the constancy of the retention rate. Lees (1953: 122) points out the difficulty, and suggests that Assyriological data, covering about 5,000 years of written records, might make it possible to check retention rate at 500-year intervals over a span of several thousand years. Hjelmslev (1958: 664), however, insists that, even if this check were to be found possible, a single case would prove nothing. In my opinion, were 5,000 years of Assyro-Babylonian to confirm the retention rate, this would be evidence of great importance; it would be important if it did not confirm the rate. Meanwhile, use of the method to determine time depths of greater than 2,000 years remains an extrapolation for which no direct confirmation exists.[8]

A third limitation concerns mathematics and statistics. Lees states (1953: 113): "The validity of the glottochronologic technique rests at present on the data and the mathematical derivation to be set forth in this paper." In point of fact, Lees's calculation of chi-square implies that "there is very probably a greater-than-chance discrepancy between our theory and our data" (1953: 121). He qualifies this by four considerations: the high numerical values involved may partly account for the value of chi-square obtained; errors in dating the word lists of the earlier stages will contribute to the discrepancy between the expected and observed values used in calculating chi-square; chi-square is not the only judge of a good fit, and, even with a high chi-square, "a theory may, within quite respectable limits, be a valuable tool for certain calculations" (1953: 121); finally, perhaps the mean-rate-constant (average retention rate) was not the best function of the individual retention rates to use.

The mathematical formulations in this paper by Lees have been reported to be unsatisfactory. In any event, it is quite clear that no mathematical or statistical *proof* exists for the contention that there is a constant rate of change for the basic vocabulary sampled by either the 200-item or the revised 100-item list. It has been pointed out that neither list has been tested in the exact form in which it is used; that tests beyond a bit over 2,000 years are not available; that the control cases from which retention rates have been calculated are biased toward a particular area and language family; and that no study has been made which would show that the apparent tendency of semantic groups to vary in retention is in fact averaged out across the whole list.

At the same time, it is important that Lees calculated the $^{9}/_{10}$-error in the mean rate-constant as ± 0.0176. That is, assuming the variation among individual retention-rates cases to be truly random, then, for a large number of samples of 13 from the entire population of languages of the world, 90% of the average retention rates for these samples would lie within 0.0176 on either side of the value Lees actually obtained (0.8048). Lees concludes: "Now if 90% of all sample means can be expected to lie within 2.2% of our mean $\bar{k}$ (average retention rate), we seem justified in assuming some considerable central tendency among the k's (individual retention rates)" (1953: 121).

It has been asked, "Has the reality of the tendency of vocabulary to change at a uniform rate been empirically demonstrated?" (Gudschinsky 1956*a*: 213; Longacre 1957*a*). In my opinion, this is not the right query. The clustering of the original 13 control cases, despite all the methodological difficulties, together with the results for Japanese, Kannada, Island Carib, and Arabic, constitute strong indication of a tendency for the basic vocabulary to change at a relatively steady rate. What should be queried is whether we have the best accessible estimate for that rate. Not until all available control cases have been uniformly analyzed for a standard list can we be sure of that estimate. In the meantime, it is fair to assume that the retention rates determined so far are close to the true rates for their respective test lists, and to use them in applications of the test lists, subject to considerations discussed in the next section. Indeed, it is important so to use them, and to check their results with other lines of anthropological evidence. Insofar as the available control cases cannot be truly representative of the culture areas of the world, our only source of confidence in the general validity of a retention rate is its consistency with other lines of evidence that may result from its application in areas from which control cases cannot be obtained.

In short, just as the justification for inclusion of particular items on the test lists must be partly pragmatic, so the justification for a particular retention rate must be partly pragmatic. The control cases give some support to those now used, and a survey of applications in the next section finds some support there also.

To sum up: The basis of glottochronology stands in need of research at each point.[9] Our understanding of basic vocabulary and of rates of lexical change has only begun. There are few studies that compare the basic vocabulary with other areas of vocabulary for rates of borrowing, of retention; there are no full-scale studies of homogeneity and persistence with regard to semantic groups. All these can be studied wherever reconstruction of the proto-forms for the items on the test list is possible (as in Polynesian and Athapaskan), so that we need not be limited to languages documented from earlier stages. There is room for a good deal of experimentation in control cases, with random and planned variation of test lists. Re-analysis of the control cases, and fresh analysis of new ones, is greatly to be desired. Two points of importance are the publication of the data for all the control cases, and the determination of reliability in obtaining equivalents and judging cognates. There are at present only two control-case studies that present both the equivalents obtained for the test list and the judgment of cognation made (Hattori 1953; Satterthwaite 1960). Finally, quite apart from the use of glottochronology for historical studies, there is the general interest of the question of basic vocabulary itself: what items comprise it, why they should comprise it, and how it is possible for them to differ from other sectors of vocabulary. As noted, there has been no real

investigation of the relation of basic vocabulary to frequency, to mention one obvious possible explanation for persistence as well as one commonly assumed attribute; one could also investigate early learning, latency, and recall for the test-list items. There has not been sufficient exploration of the size of the basic vocabulary, judged on other than least-common-denominator grounds, nor of its definition in terms of semantic fields as well as items. Referring to the two main questions of the basis of glottochronology, the existence of a universal basic vocabulary, and the constancy of its rate of change, Householder writes: "It may well be that the next few years will bring, not merely empirical confirmation (or rejection) of these two assumptions, but structural, psychological, or sociological arguments from which they can be derived (or disproved) as theorems" (1957: 160).

While we need much more study of the basis of the method, we should continue to apply it, subject to known limitations and check by other data. Such applications help to refine the method, and shape practically the confidence we have in it, while at the same time their results add to our knowledge both relative time depth and approximate dates.

THE APPLICATION OF GLOTTOCHRONOLOGY

THE PROBLEMS of application are implicit in formulas of the type $t = \log C / 2 \log r$, and the instruction, solve for t. Such formulas are used to determine divergence time, when two related languages are compared, and the fraction of corresponding test-list equivalents that are cognate between them is calculated (C), the appropriate retention rate for the test list (r) being assumed. (As we shall see in discussing the evaluation of divergence times, t must be taken as *minimum indicated time* in the particular formula cited above, and Swadesh[☆] recommends that, to avoid misunderstanding, it should be explicitly written *min* $t = \log C / 2 \log r$.)

Problems of application have proved separate from the problems of the basis of glottochronology. No matter how precise the basis, application introduces novel factors that may affect results. These include reliability in filling out the test list; reliability in determining glottochronologic sames; evaluating divergence times found; sub-grouping and spatial alignment; and fitting the results into other knowledge. (The use of the test lists to formulate hypotheses of genetic relationship, or to prove relationship, is discussed below as part of lexicostatistics.)

RELIABILITY OF TEST-LIST EQUIVALENTS

Perfect reliability in filling out the test list would imply that each and every investigator of a language would obtain the same single, simple, clear equivalent for each test item. There are two major kinds of adaptation of the list which raise questions of reliability: (*a*) when the investigator chooses among alternative equivalents for a test item; and (*b*) when the investigator chooses supplementary items for which to obtain equivalents. Ideally, of course, neither type of choice would be necessary. In practice, this seldom occurs, because of intrinsic or extrinsic factors. Primary among intrinsic factors is the difficulty of finding or selecting an equivalent form because of the semantic patterns of the particular language (Hoijer 1956*a*; Hattori 1953: 74; Brinton 1891: 334; Hjelmslev 1958: 664), and because of variation among informants. The principal extrinsic factor is dependence on inadequate material, from languages no longer spoken or imperfectly reported. When such factors make it necessary to adapt the list, one has recourse to three types of procedure: eliminate uncertain or unobtainable items; choose between uncertainties; supplement the list.

As noted before, Swadesh first recommended that, "Where it is impossible to find a single equivalent, omit the form" (1952: 457), in hopes that omission would seldom be necessary. But if any sizable number of items is eliminated, the range of error for the estimated divergence times increases, and the time depth penetrable by long-range comparison decreases. Thus Gudschinsky (1956*b*: 179) recommends a choice among alternatives by some random method (such as flipping a coin), and many studies have not hesitated to adapt the list by supplementing it. Elbert, starting with the 165 items of the pioneer Salishan study (Swadesh 1950), alters and extends the list to comprise 202 items, judged appropriate to the Polynesian area, and chosen in terms of semantic diversity and the convergence of English and Polynesian semantic fields (Elbert 1953). Because of limited material from which to obtain equivalents, Hirsch omitted some test items and supplemented with others in his Eskimo study (1954: 825). For the same reason, Taylor omitted some items and added others, "but none of these can be regarded as cultural for the peoples and localities concerned" (1952: 227). Hoijer added 22 items from the second or supplementary 100 in the revised edition of Swadesh's *Amerindian Non-Cultural Vocabularies*, since he found only 78 from the first or diagnostic 100 to be usable (1956*b*: 219).

We must ask how best to adapt the test list, when necessary; how appropriate are retention rates to adapted lists; how comparable are results obtained from different lists?

It must be pointed out that apparent ease in filling the test list may mean that limited knowledge is concealing the problem of adaptation. This is borne out by a personal communication from Ward Goodenough. A test list was completed on Truk in the early stages of linguistic study, and again later in the light of greater knowledge of Trukese; the two lists differed notably. As to semantic appropriateness, Hoijer suggests that "apparent lack of difficulty in translation results from lack of careful definition. . . . The difficulties we have noted come to light only after careful semantic studies, based on far more material than is ordinarily required for structural studies" (1956*a*: 58–59). With regard to choosing the common or usual form, Olmsted (1957) has shown that results can differ considerably, according as to whether a cognate, or a non-cognate, is chosen in all doubtful cases; both divergence times, and the pattern of sub-grouping, among ten Bantu languages

were affected. Olmsted used sources that simply listed a number of native forms after an English gloss, but the problem is not obviated if a printed source or an informant supplies only one native form for a test-list item. The question of semantic appropriateness is begged, and while there is a probability that the first equivalent volunteered by an informant (or given in a dictionary) is the most common one, it is not a certainty. Clearly a specialist's knowledge is required if the test list is to be validly filled, in complete conformity with the instructions. Ideally, two or more specialists would complete the list as a check on reliability. (So also Driver [5].☆)

A special problem is that of semantic ambiguity. Even if the meaning of a test-list item is clearly understood, it may point to more than one equivalent in a particular language. This problem will be taken up immediately below. But the meaning of a test-list item may itself not be clearly understood. It may point to more than one referent for investigators. Thus Swadesh may intend the item "claw" to denote the animal counterpart of a fingernail, but someone else may take it to refer to the entire clawed foot (see Gudschinsky [3].☆) The mimeographed copies of the list distributed by Swadesh have carried instructions to help specify the referents of the list items, but the experience of those who have used the list should be pooled to make certain that the range and nature of such ambiguity is fully understood. Editions of the list, or of similar test lists, should always be accompanied by the semantic specification that such experience shows to be necessary.

The problem of variation in response among informants will be discussed below, in connection with the problem of the effective lower time limit of the method, under "Sub-grouping and Spatial Alignment."

Adapting the List. When one chooses among alternative equivalents, the essential thing is to follow fixed criteria. If the same positive criteria are followed in all cases, results will remain directly comparable. These criteria may be of three sorts: randomizing, diachronic, synchronic. A randomizing procedure, such as flipping a coin, is recommended by Gudschinsky (1956*b*). A diachronically oriented procedure would be to choose a known cognate (retention) in doubtful cases, since this would increase the number of items available for long-range comparisons. A synchronic procedure would prescribe equivalents in terms of structural or semantic rules. These might be general, or particular to the language family. Where Athapaskan languages have suppletive singular and plural stems, Hoijer chooses the singular form; this rule could be followed for all such cases. (On the other hand, the plural form might prove preferable in some other language family.) At present, it is important for individual studies to specify criteria, and not magnify the difficulties. In his critique of the method, Hoijer (1956*a*) presents 28 problem sets, or points of difficulty, which he encountered in filling out the 100-item list for Navaho. These sets involve 39 of the test-list items. In his study of Athapaskan, however, Hoijer (1956*b*) was able to resolve 13 of the problems (involving 19 of the items). Thanks to his detailed analysis, the only one of its kind so far, the remaining 15 problems (and 20 items) can be investigated for guides to positive criteria.

Two of the problem sets are comprised by the pairs *this, that* and *who, what*. In each pair, only one item causes difficulty. *This* and *who* have single Navaho equivalents, but *that* and *what* have more. As a semantic rule for alternative *that*-forms, I suggest that the form whose meaning is "most proximate to the speaker" be considered the most appropriate partner of *this*. Among Navaho *what*-forms, no clear semantic distinction emerges, and here a random or diachronic procedure must be used to select an equivalent; the same is true of *big*, the themes for *give* differing as to object-category implied, and the choice between generic terms for hard, rough *bark* and soft *bark*. The most interesting cases involve overlap of meaning between potential entries. Hoijer resolves this in the case of *head* and *hair*, choosing for *head* a form that denotes both head and human hair, and for *hair* a form that refers to animal hair, fur, or wool when used independently, but also to human hair in certain compounds. The implicit principles are: (*a*) if a form is possibly equivalent to more than one item, but the only possible equivalent for but one item, select it for that item; (*b*) if two forms are possible equivalents for an item, but one has a range of meanings overlapping another test item, and one does not, select the latter. These principles also can resolve the problem of Navaho forms for "man, mature male," on the one hand, and "man (as opposed to woman), person (as opposed to animal), Navaho (as opposed to non-Navaho)," on the other. The latter should be selected for the test item *person*, the former for the test item *man* (*male*). In the case of "small birds" versus "flying beings," the rule can be to select the included, rather than the including, equivalent (for *bird*). For *small*, "be small in size, dwarfed" should be selected rather than "small one; child, young one"; this would seem to follow from the intent of the list, but, if not, can exemplify a semantic rule: choose the descriptive rather than the denominative sense for all items tabled as "Descriptives" by Swadesh (1955*b*: Table I, sect. t). The former seems also the least complex in the Navaho case; a rule of selecting the form with the least complex, least specific, most neutral range of meaning is implicit in Hoijer's selection among equivalents for *see* and *black*, and also can take care of *dry* and *know*. Thus, of the Navaho difficulties there remain only the fact that the generic forms equivalent to *seed* and *eye* are the same, and that the forms equivalent to *walk* and *come* differ only in modal inflection. (Though *grease* and *good* are listed as problems, Swadesh's instructions imply the choice of "fat of meat or of the body" for *grease*, and the sense of "useful" for *good; root* has three possible equivalents, which are difficult to analyze, but contain a common stem that can be entered.) For duplications, I suggest, along lines considered but not adopted by Swadesh (1955*b*: 126), that the items be kept and the forms entered; a final decision as to elimination or scoring can be made in the light of data from the entire language family.

Another synchronic procedure is to choose the most frequently occurring item from among possible equivalents. This has been done in a particular way by Satterthwaite (1960). In his study of the retention rate between contemporary Meccan Arabic and that represented in the Qoran, he collected all Arabic words that

he recognized as equivalent to the 100-item list. (This recognition stemmed from several sources: eight years of contact with spoken Arabic, acquaintance with the Qoran and ancient and modern literary Arabic, and study of speakers of modern Arabic from Khartoum, Syria, and the Arabian peninsula.) He then checked against a Qoranic concordance. There are three difficulties here. First, as Satterthwaite notes, such a method may omit equivalents actually present, but not independently known to the investigator. Second, no assurance is given that a form suggested from a non-Qoranic source, although found in the Qoran, is found there in the range of meaning of the pertinent test-list item. The third difficulty is general. The method assumes that forms marking the meanings specified by test-list items not only occur in every language but occur more frequently than possible near-equivalents (whose meanings may be adjacent, included, or overlapping). We have seen that the correlation of most frequent items in general, with test-list items, is notably less than perfect in several languages, and this may well hold within a range of forms suggested by a test-list item. Indeed, Satterthwaite notes that occasional investigation of the exact meaning of a word (by reference to the Qoran) led in several instances to rejection of a more frequent equivalent in favor of one less frequent. This bears out our finding that the criterion of frequency and that of universality (which has heavily influenced the construction of the list) overlap, but do not coincide. In short, the investigator must seek to control the semantic appropriateness of test-list equivalents.

Other problems, and other possibilities for meeting choice situations with definite rules, will be encountered in other languages. It is important that these be investigated and codified, so that equivalents can be obtained in standard ways that will keep the test list intact. As Kroeber concludes, "Incomplete lists can affect retention rates; and substitution of word pairs outside the list to fill lacunae in it may make matters worse" (Kroeber 1958: 457; also 1959). (Dyen☆ also explicates the loss of reliability with incompleteness.)

As for supplementing the list when necessary, I have suggested that the basic vocabulary of a language is larger than the test list. A supplementary item may be justifiable as consistent with the general criteria for selecting the diagnostic items of the test list in the first place. It may be the counterpart in a particular linguistic and cultural setting of items that are basic in the setting of the control cases. This should be checked by matching the behavior of supplementary items with the behavior of original test items in application, and, if possible, with the behavior of replaced items in the control cases. Marshall (1956: 84) has made an interesting proposal for modifying the Swadesh list so "that there be a series of alternate terms within each word class from which the analyst could pick out those terms which seem significant and well distinguished by the language of the people of his area. The main proviso is that he ensure that the same percentage of terms within each class is used" (along lines suggested in a following chapter). Thus, it may be possible to use supplementary items without making matters worse.

In any case, the advantage of having test items represented by equivalents, rather than dropping items, is great enough to encourage considerable research into means of meeting the difficulties. Besides developing rules for special cases as they arise, we should know the effect that different procedures would have on the control cases. Here again, it is not enough to point out difficulties; what is needed is help in removing them.

Appropriateness of Retention Rates to Adapted Lists. It has been pointed out that all uses of the 200- and 100-item lists are, in effect, cases of using retention rates for adapted lists, since neither list has been tested as a whole in the form in which it is used. Other things being equal, it would seem reasonable to consider the established retention rates appropriate to slightly modified test lists, but we have no criterion for "slightly." Thus, divergence times resulting from even the intact use of the 200- or 100-item list must be carefully designated "approximate" on grounds of adaptation alone.

The consistent use of any sizable modified test list still can give good indication of relative divergence times within a family of languages. Kroeber (1958) and Driver (personal communication) consider this a valuable result quite apart from attached dates. As to approximate divergence times, calculated with a retention rate, our confidence must decrease in proportion to the amount of modification the test list has undergone in a study, unless acceptable reasons are given for the modification. (What such reasons might be has been suggested above.) Future analysis of control cases may make it possible to determine appropriate retention rates for quite a range of adaptations of the list. Hirsch says of his Eskimo adaptation, "There is no way to demonstrate that the same constant would work for the new list other than going through all the labor involved in obtaining the original constant. However, it is quite feasible to use the results obtained through the new list as relative dates with no specific time depth implied" (1954: 825). As noted before, even if one were willing to labor, the data for the control cases are not published. If they were, together with calculations of persistence rates per millenium for all items on the test lists, the appropriate rate for a modified list could be found.

Swadesh has made possible an indirect check on the appropriateness of a retention rate for a modified list. His scoring (Swadesh 1955*b*) gives persistence scores for 215 items in terms of total number of decades covered by eight control cases, that is, percentage of total time per item, whereas a retention rate shows percentage of items per period of time; there is no exact relationship such that an adapted retention rate could be determined from his scoring. But Athapaskan represents an example of what can be done. Hoijer (1956*b*) uses a modified test list, 78 of the items of which are from the revised list with a retention rate of 86%, and 22 from the supplementary list. The average persistence, in decades, of 92 items on the revised list can be determined from Swadesh's Table; it is 73.56%. The average persistence of 90 items from the list used in the Athapaskan study can be determined; it is 73.41%. This is an indirect indication that the 86% retention rate is appropriate for the modified Athapaskan list.

Comparability of Results from Adapted Lists. It is clear that at the present time the various adaptations

of the test lists are not known to be strictly comparable. In most cases, the adaptation has been *ad hoc*, or at least not publicly attested. One's judgment of the comparability of results of different studies must be proportionate to the degree and kind of modification in each. In principle, comparable adaptations can be made, but research into the method has not gone far enough to make this possible on the basis of a standard procedure. All applications should avoid *ad hoc* modification; adaptation in terms of explicit rules that strive for generality facilitates comparison with the results of other studies, and helps to refine the method.

DETERMINING GLOTTOCHRONOLOGIC SAMES

When two completed test lists from related languages are compared, the pair of forms corresponding to each item are scored as cognate or non-cognate in the sense of the comparative method. For English and German lists, *hand* and *Hand* would be scored as cognate, *head* and *Kopf* as non-cognate. The fact that German retains a cognate of head (*haupt*) would be irrelevant, since the procedure compares not merely forms that are retained, but forms that are retained in the same central or principal meaning. Forms that are not so retained simply will not appear at the same point on the list. Thus, what have been referred to as "glottochronologic cognates" are a sub-set of all the cognates between two languages. Some have found this special use of "cognate" confusing, and I propose that this sub-set be designated *glottochronologic sames*. (Cf. Dyen's☆ classification of *comparable*, *determinate*, and *positive* pairs. The latter are equivalent to *glottochronologic sames*.)

In principle, glottochronology should be applied only after the comparative method has prepared the way. In practice, a number of scholars have not hesitated to proceed without this safeguard. This introduces a variable factor of judgment, when there is an imperfect fit of sound correspondences; much glottochronology, like much of the classification of the world's languages, has relied on some criteria of inspection. Olmsted stresses the danger of this in the hands of one who is not a specialist in the languages concerned, and concludes: "In such circumstances, it is clearly a disservice to the theory to attempt to apply it prematurely. The conditions in which the hypothesis is said to be operative are simply not present and the likely result is a weakening of confidence in it which is unnecessary and unwarranted" (1957: 841).

The problem of skewing as a result of imperfect determination of cognates is capably discussed by Gudschinsky (1956*b*: 182, 183, 184, 200). Criteria for judging probable cognates, or "correlates," have been used and discussed by Gudschinsky (1956*b*: 184), Bright (1956: 42–43), Fairbanks (1955), and Taylor (Taylor and Rouse 1955: 106–107). Bright notes that Fairbanks' counts of known cognates versus similar non-cognates in pairs of related languages tend to agree "because the differences between the methods cancel each other out. I draw from these facts the conclusion that comparison of vocabularies on the basis of similarities may be, though less reliable than comparison by correspondences, nevertheless a helpful indicator of the general relationship between the languages involved" (1956: 42–43). Gudschinsky believes that her criteria "will in general discover the pairs of words which may be considered as probable cognates within a margin of error not great enough to invalidate the method or render the results useless, even though in any one particular instance, the conclusion might not reflect the actual historical facts" (1956: 181).

Swadesh (MS.), however, reports: "It would seem here and from other experiences of or known to the present author that similar non-cognates and non-similar cognates do not always cancel each other out. The question depends a great deal upon the complexity of the phonologic facts in the particular pair of languages and to a considerable extent also on the personal equation of the person making the study. There is often rather little discrepancy between early and later lexicostatistic counts, when the phonologies involved are fairly obvious and when the researcher takes some steps to verify his first phonologic suppositions at least in part. However, with difficult problems of phonetic equivalence and structural fusion, like those encountered in Amuzgo and Trique, even a partly developed phonologic theory may not suffice."

In the studies cited above, the order of increasing rigor of criteria seems to be Fairbanks, Bright, Gudschinsky, Taylor; it is not known, of course, that greater strictness is more successful in approximating the actual number of cognates. Note that Fairbanks' comparisons of Czech, Russian, English, German, French, and Armenian suggest different quantitative relationships between the proportions of true cognates and similar non-cognates, for different degrees of divergence. The two language pairs with a high percentage of cognates (60%, 74%), and thus low divergence time, have few discrepancies between forms that are cognate and forms that are similar. The three language pairs with about 25% cognates show many more items that are cognate but not similar, than does the preceding group; the percentage of cognates that are not similar (33%–45%) is higher than the percentage of similar items that are not cognate (about 25%). The remaining three pairs with about 17% cognates agree with the middle group in having many dissimilar cognates, but the proportions of similar non-cognate items and cognate non-similar items are the same. Any further knowledge of such relationships would be valuable.

Grammatical analysis is an important factor here. Thus Swadesh has counted Tsimshian *-hetk*^{w}: Chinookan *-tx*^{w} "stand" as cognates, but the *-tk*^{w} of the Tsimshian form is a suffix of very general meaning, the stem "stand" being *-he*.

The use of probable cognates introduces the possibility of overestimating divergence time by counting less than the actual number of cognates, or of underestimating it by counting more, unless further study bears out the suggestion that missed and false cognates tend to balance each other. Whether probable or known cognates are counted, some factor of judgment may enter; thus Fairbanks' counts for English-German and English-French differ somewhat from those by Lees (Fairbanks 1955: 118, n. 11, 13; Lees 1953: 122, 123). Reliability requires that judgments of cognation be made independently by more than one scholar whenever possible. The common standards of science would seem to require that judgments of cognation be made

available for public inspection, but only some have done so (Hattori 1953; Taylor and Rouse 1955; Gudschinsky 1956*b*; Hoijer 1956*b*; Satterthwaite 1960; Swadesh MS.) In other published studies the judgments are unreported, and no criticism or replication is possible.

I would agree with Gudschinsky and Bright that some useful results are possible on the basis of probable cognates. Swadesh (MS.), for example, although clearly presented as a tentative formulation of Mixtecan relationships with regard to the Oto-Manguean hypothesis, is a step forward from the impressionistic judgments that have played a large part in the development of this broad classification. The use of probable cognates, however, again contributes to the approximateness of divergence times by adding a source of variation. Their use, especially when published judgments of cognation are lacking, again limits the comparability of the results of different studies.

In sum, approximations are interesting and often useful, both for their results and for their methodological implications, but it is safest for glottochronology and for historical anthropology when the method is applied to languages the correspondences of which have been thoroughly studied by the comparative method. Such a case affords maximum control over all the factors that introduce variation into the divergence times.[10]

EVALUATING DIVERGENCE TIMES

Even in the ideal case, glottochronologic dates "provide only an estimate of the most probable date, together with some estimate of the probability of any given deviation from such a date" (Gleason 1955*a*: 343). Gudschinsky (1956*b*) provides a procedure for calculating this range of error. In practice, as we have seen, a number of considerations require us to take present glottochronologic dates very much as approximations. The principal questions about the divergence times concern the assumption of independence among diverging languages; correcting for lack of independence; and the comparability of results.

The Assumption of Independence. The original formula for application, $t = \log C/2 \log r$, assumes that as two languages diverge from their common ancestor, they retain or replace equivalents to the test list quite independently of each other. That is, this formula assumes that C, the fraction of retentions that will be common to the two languages, is the product of the fractions each will retain. Since both languages will have diverged from the common ancestor the same length of time, the fraction (c) retained in each will be the same, and hence $C = c^2$.

Other situations are theoretically possible. Suppose that the two languages retain exactly the same test-list equivalents, and replace exactly the same. Then the above formula would not hold. C would be equal to c, and, to result in the same time depth, the formula would have to be $t = \log C/\log r$. Or, suppose that one language replaces none of the same items as the other. (This could continue until each had replaced 50%, or for something over 4,000 years, assuming the 100-item list and 86% rate.) Then C would be equal to $(2c-1)$, and the time-depth formula would again have to be different; one version would be

$$t = \log (C + 1) - \log 2/\log r$$

or

$$t = \log (C/2 + 1/2)/\log r.$$[11]

Thus, the appropriate formula for calculating time depth depends on the relation between the retentions in the diverging languages, and a formula may give time depths that miss the mark if the facts do not correspond to the assumption on which it is based. Of course, neither complete identity nor complete difference in retentions is a likely event. But complete independence is also problematic.

When divergence is not completely independent, what factors are likely to cause departure from independence? Principally three: *contact*, *drift*, and *dregs*. If two languages maintain some *contact* after divergence begins, or resume contact after separation, their retentions and replacements are likely to be alike more than chance would indicate. As to *drift*, since the diverging languages begin with more or less the same balance of internal forces for change, they are likely to change in similar ways to some extent even without contact. (On drift in Indo-European vocabulary, see Meillet [1937: 379, 423ff.] and Hattori [1]☆; Hattori [1953: 76] discusses the effect of drift.)

By *dregs*, I tag the factor previously discussed with regard to the validity of the test list, namely, that if some items are more persistent than others, their proportion will increase through time. (Cf. Hattori [1].☆)

All these factors lead to underestimating the true divergence time, since divergence time is inversely related to fraction of shared retentions. Ideally, the paired forms counted as shared by two related languages should be entirely cognates independently retained from their common ancestor. The actual count of shared forms is likely to be larger, containing a mixture of sames due to independent retention, post-separation inter-influence, and drift.

Correcting for Lack of Independence. The problem of underestimation has long been recognized (Swadesh 1952: 461; 1955*b*: 123) and is stated by Hockett (1958: 534), although some at first doubted it. A number of ways to cope with it have been devised, both evaluative and technical.

Three practical considerations in evaluating time depths are: (1) evidence of complete geographic separation between two languages implies that contact has not affected independent divergence, and the resulting time depths are closer to the true ones than otherwise; (2) within a family of languages, the greatest time depth between a pair of languages is likely to be closest to the true time depth of the whole; (3) when a series of successively greater values for time depth level off after attaining a given maximum, this may represent the actual time depth, except, of course, for the effect of variables other than contact.

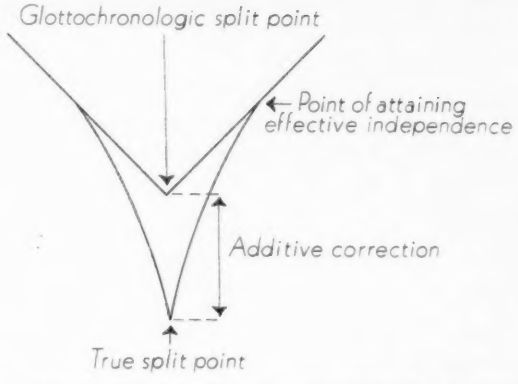
For the 200-item list, Lounsbury has prepared a revised nomograph scale; it is adjusted, from the one given in Gleason (1955*b*: 88) and in Hockett (1958: 533), on the basis of assumed real time depths for certain Indo-European control cases (Lounsbury 1957, courtesy of W. C. Sturtevant). A variety of experiments with nomographs for the 100-item list could be undertaken and would be valuable.

When the forms for the test list can be reconstructed to the proto-language (as undertaken by Elbert [1953] and by Marshall [1956] for proto-Polynesian), much of the problem may perhaps be circumvented. Divergence time may be treated as a single line, a quasi-control case, and items shared between the modern and the proto-language may be counted. This would bypass much of the effect of drift and contact on daughter languages, provided sufficient rigor is exercised in the choice of items to reconstruct.

Gudschinsky (1955: 141–42; 1956*b*: 206–207) advocates that, to avoid being misleading, one should separate glottochronologic results from time by the concept of *dips*, which expresses the degree of objective lexical relationship shown by the test vocabularies: $d = 14 \log C/2 \log r$. This is equivalent to correcting the denominator so that it reads $1/7 \log r$, but nothing is said of years or dates. The correction is large to provide greater gradation. Such a step fits Kroeber's remark that "the greatest danger in lexicostatistics is its involvement with chronology, which is its weakest point, though also the one of widest appeal" (1958: 455). Gudschinsky notes the practical value of the dip measure, apart from time, in making decisions about homogeneity of speech areas for vernacular schools, production of literature, and so on. But these considerations loom largest in areas of closely related dialects. (Nor does the concept of "dip" bypass the problem of using an exponential curve, as pointed out by Gleason.☆) For the relationships between different languages, and for the problems of culture history, some stab at time should be made. Recognition of *minimum* times, and of range of error, stressed by Gudschinsky, helps obviate the danger of taking results uncautiously as absolute dates. Those who may be misled should be instructed. Exploration of the sources of variation in time-depth estimates is of interest to culture history, quite apart from whatever contribution it may make to glottochronologic accuracy.

Gleason☆ has assessed the nature of the three factors of drift, contact, and dregs, and the appropriate measures for correction, more carefully than most. On drift, he writes (see p. 11, above):

> Assume that two languages do in fact separate sharply at some point in time. Assume further my assumptions about probability of change. Then it follows that, at the time of separation, every item has a certain probability of change, the same in each separating language, and that many of these differ at that time from the mean probability for that item. Viewed in the light of the means (what we must do in glottochronology), the two languages will operate as though they were less than fully independent, by virtue of the identical probabilities of change of each item in the two languages. But as time goes on, the probability of change of each item will vary (by premise 2). The expected outcome of any such process would be increased randomness of the distribution of departures from means between the two languages. In short, the process of change in this event should be to approximate independence increasingly closely. After a certain lapse, we could assume that the process of change in the two languages would be effectively independent. From this point onward, the formulae (if they were otherwise correct) would require no adjustment. Thus a correction should be applied only to the first period of separation. Multiplication is then incorrect.
>
> We may graph the relationship as follows. Right angles represent independence. Lesser angles represent a tendency to common innovations:



> It *might* be that this effect can be calculated, however roughly, and a suitable correction determined. This correction would necessarily be additive, not multiplicative.

On error from contact, Gleason observes:

> What this means is that glottochronology can calculate only a time at which a sharp split would have produced a result comparable to that produced by some earlier gradual split. (This would be what is labelled "true split point" in the diagram.) This may have very little chronologic reality, but it seems to me that it does have a great deal of historical usefulness, i.e., that it measures something worth measuring.

As to error from "dregs," Gleason asserts: This "can be and must be taken care of by a recalculation of the curves of retention versus time depth" (nomographs).

Gleason thus analyzes three factors that affect retentions independently, suggesting distinct modes of correction for two. Other approaches to this problem have suggested some single mode of incorporating correction into the formula itself.

We noted that Swadesh specifies that the formula read: $min\ t = \log C/2 \log r$, that is, minimum indicated time. The results in Swadesh (1958) are so given. He has also proposed (1955*b*) the definition of a variable, $\bar{s}$, as average degree of separation between a pair of languages. This would have a value of 1 for complete independence (and *min t* would equal true *t*), but become smaller as lack of independence increases. Divergence would then equal $\bar{s}t$, and the formula would read: $t = \log C/\bar{s} \log r^2$ or $t = \log C/\bar{s}\ 2 \log r$. This helps conceptually, Swadesh points out, but in the individual case no way of calculating $\bar{s}$ is known without knowing *t*. Thus a control case, where time depth was known, could be treated as a case of application. Instead of comparing parent and daughter language, one could compare pairs of daughter languages. The discrepancy of these results from the true time would give the value of $\bar{s}$, and, in the individual case, or in a series of such cases, worthwhile insight into the divergence of the family quite apart from time might result. But unless a standard value for $\bar{s}$ could be determined, true cases of application would not be helped. It is almost certain that the true value of $\bar{s}$ varies significantly from case to case, but perhaps a *minimum* value of $\bar{s}$ could be determined as a standard factor in the formula. This would provide a correction for error arising from contact, one which might be a full correction in some cases, but undoubtedly partial in others.

Hattori (1953, and cf. his [1]☆) has proposed a general mode of correcting for lack of independence, covering in his conception the joint effect of both drift and dregs. After his control-case study of proto-Japanese and three modern languages, he treated the modern languages as a case of application. Seeking to correct the resulting underestimation of the true time depth of the proto-language, he estimated a German-English time depth of 1,800 years, and on this basis determined that the figure 2 in the formula should be replaced by 1.411. Most easily expressed, this is to say that the formula should read $t = \log C/1.4 \log r$. (This is, of course, equivalent to $1.4\ t = \log C/\log r$.) Lees (1956) terms this a "jiggle factor," J, implying that it is an unscientific tinkering to make things fit. Quite the opposite is true. Hattori's analysis in effect treats the power of r (or the factor by which $\log r$ is multiplied) as a theoretical variable in the glottochronologic formula. It is a variable having to do in some way with the matter of chance independent retention of cognates, or departure from this. It has of course a relation to Swadesh's proposed $\bar{s}$, namely, $\bar{s} = J/2$. Thus Hattori's German-English estimate and consequent application of the figure 1.4 to Japanese both imply a .7 value of $\bar{s}$. And his corrected formula is equivalent to $.7t = \log C/2 \log r$, that is, to multiplying the minimum times by .7. Rather than term it a "jiggle" factor, let us refer to it as an *adjustment* factor, i.e., adjustment for degree of independence, if we wish to retain the symbolization "J."

Lees observes that Hattori's value for J gives a better historical fit for the split between Turkish and Azerbaijani (A.D. 1180) than that in Lees (1953); the French/English split of 2350 B.C. may seem rather remote when viewed from the Trager and Smith assumption of Indo-European splits beginning about 2500 B.C., but many feel that this latter date gives too short a period (Lounsbury, personal communication). In a discussion of Rea's Romance study, Kroeber (1958: 456) multiplies the results by 2.2 to obtain figures with good fit, but we shall see later that such a large adjustment is not necessary. (Kroeber's 2.2. is equivalent to a Hattori J of .909, that is, of using a formula, $t = \log C/.909 \log r$.)

Perhaps the best way to conceptualize Hattori's approach is to write the formula for application as $t = \log C/x \log r$, where x is an as yet uncertainly known value, or as $t = \log C/J \log r$. Obviously it is necessary to examine as many control cases as possible in order to discover the range of values that J may take. As with the Swadesh $\bar{s}$, it is possible that a standard value giving a minimum correction can be established, such that the time-depth estimates obtained from the formula will be closer to the true ones.

Here I can now cite the work of Milke,☆ who has kindly sent me a copy of his monograph, *Theorie der kulterellen Ähnlichkeit* (1955), which I had been previously unable to consult. Milke develops a mathematical theory of cultural similarity and change which includes application to language. A major feature of his work is to show that different mathematical models are possible for the evaluation of the relationship between amount of cultural similarity, various processes of culture change, and time. In section 6 (pp. 19–20) of this monograph he presents three formulas for percentage of similarity (our equivalent here is C, the count of shared forms). Formula (1) applies to similarities exclusively the result of diffusional cumulation, formulas (2*a*) and (2*b*) to similarities exclusively the result of retention from a common ancestor, and formula (3) to similarities jointly the result of cumulation and retention (when the ultimate territory of the groups has been directly occupied). In section 12 (pp. 40–45), such considerations are used to distinguish three possibilities, and corresponding formulas for calculations from the glottochronological test lists: (1) when the lists contain shared forms that are exclusively retentions from the proto-language (or, I must add, when only such retentions are counted); (2) when the lists contain both retentions from the proto-language and shared innovations retained by two languages from a period of unity subsequent to the split of the proto-language of the family and preceding their present separation (or, again, when only such retentions are counted); (3) when all shared forms, including both retentions and borrowings, are counted.

Milke states that in order to judge the results of a glottochronological study, it is essential to know which of these last three situations is the case, and thus which formula for calculation must be used. (I do not reproduce his formulas, since the entities in them depend for intelligibility upon preceding theoretical argument.)

We have already discussed the dependence of the formula on the facts of a case with regard to independence of retentions. Milke's treatment brings in direct measurement of degree of relationship, of similarity due to historical connection whether inherited, acquired, or both (cf. Ellegård 1959), and conditions of separation such as migration, immediate occupation of the territory now inhabited, and so on. This suggests that our previous statement should be made more general, thus: "The formula for calculating time depth depends on the relation between the diverging languages, and a formula may give time depths that miss the mark if the facts do not correspond to the assumption about this relation on which it is based." The relation between the diverging languages in turn comprises all those factors—social, cultural, spatial, both specific to the case and general—that affect the number and identity of retentions.

As to Milke's three formulas, if borrowings are known, they are excluded, so that the third formula does not apply in a glottochronologic context. Since calculations are between pairs of languages, there is no pertinent difference between the assumptions of the first and second formulas, so far as the correct time depth for that pair is concerned. (Not so, of course, for the time depth of the family, if comprising more than the pair.) Our discussion of absolute identity and difference in retentions (p. 19) indicates that still other possibilities must be taken into theoretical account, if the full repertoire of possible situations and their appropriate formulas is to be amassed. Thus I consider the thrust of Milke's work important, since it brings language and the rest of culture within a single theoretical perspective in approaching time perspective, but a first inspection

of his specific proposals for glottochronology finds them not helpful. Such mathematical considerations, however, help us conceptualize and explore the range of situations and variables.

To sum up: There has emerged a widening recognition that cases of application (and the real world of language change) involve variables beyond those of time, fraction of common cognates, and an average retention rate. The focus of such recognition is on the number of retentions and the formula for calculation, but behind these loom a variety of socio-cultural and statistical factors. Gleason sorts out drift and dregs with proposals of separate modes of adjustment; Swadesh proposes a variable concerned with contact ($\bar{s}$); Hattori proposes to treat an element of the formula as a variable concerned with drift and dregs together (J); Milke brings in considerations of spatial factors as well. In theory, and in basic analysis, it is essential to discriminate the various factors carefully. In future practice, it is likely that a single, composite variable for adjusting the formula for application will be adopted. Likely it will be a value of Hattori's J, set at a minimum value, and perhaps most easily handled by using the formula $Jt = \log C / \log r$. But this is speculation. The further sorting out and empirical study of the factors affecting independence of retention is of prime importance to the refinement of glottochronology.

Comparability of Divergence Times. Many factors might make divergence times resulting from different applications incomparable: difference in test list used, difference in judgment of cognation, difference in material selected from within a family. Thus the time depth calculated for Eskimo-Aleut has changed with successive changes in the material used, the test list, and, perhaps, judgments of cognation. (Bergsland [1958: 656] disagrees with eight etymologies used in the study by which Swadesh calculated about 3,000 years, using Aleut and proto-Eskimo.) Swadesh (1958: 672) now gives a time depth of 46 centuries minimum divergence as between Aleut and Southwest Greenlandic, considering this a measure of the fullest divergence in the family, but Bergsland (1958: 656), "applying the rules strictly," reports about 6,000 years for his calculation.

In some cases, to be sure, different divergence times seem not to affect the general pattern of sub-grouping. For Mixtecan, Gudschinsky (cited in Longacre 1957*b*: 8) uses both the 200-item and 100-item lists; the divergence times for the latter are all higher, but in each case the depths between Trique-Cuicatec and Trique-Miztec are almost identical and somewhat more than that between Mizteco-Cuicatec. This agrees with the independent results of Arana (reported in Fernandez, Swadesh, and Weitlaner 1959: 56), although all her divergence times are lower than Gudschinsky's. Arana's Mizteco-Cuicatec result, however, is sharply lower, ten centuries less than the other two time depths, whereas Gudschinsky finds a difference of only five centuries less on the 100-item list. Thus Arana's results do not, as Gudschinsky's do, bear out Longacre's contention that Mixtec, Cuicatec, and Trique reconstruct roughly in the same time depth without any obvious sub-groupings. (See Swadesh [1960] for evaluation and support of Arana's results.)

For Athapaskan, calculations with different retention rates and somewhat different lists have given the same general pattern of sub-grouping (Hoijer 1956*b*; Hymes 1957*b*). Hoijer used a 100-item list, composed of 78 items from the diagnostic 100-item list and 22 items from its supplementary list, calculating with the 81% retention rate. Hymes calculated with the 86% retention rate for both the same 100-item list and for only the 78 items from the diagnostic list. Moreover, insofar as individual comparisons differed between the 100-item and 78-item lists, they showed the Northern and Apachean groups closer to each other than either is to the Pacific Coast group, a conclusion confirmed from other data. Thus a discrepancy between applications to the same group can be a source of historical insight, if the source of the discrepancy (in this case 22 supplementary items) is known. Again, however, a calculation by a different scholar for the same language family gave a different result. Using a 100-item list, Swadesh found 64% items cognate between Navaho and Chipewyan, whereas Hoijer found 77% for his 100-item list. The Swadesh figure gives 15 centuries minimum divergence (Swadesh 1958: 656), whereas the Hoijer figure gives 864 years divergence with the 86% retention rate, and this time depth does not change if calculated only for the 78 diagnostic items.

Milke (1959) uses Hoijer's data to elaborate a new sub-grouping and chronology which gives a different pattern. Among other points, he finds a closer relationship between the Northern and Pacific Coast groups than between either and the Apacheans. His mathematical approach is of interest, including his calculation of "corrected" figures for the retentions of individual languages. Unfortunately his conclusions seem to be vitiated by an early step in his method. Since this method appears to provide a general way of gaining the advantages of a quasi-control-case approach, and gives quite different results from the usual method, something must be said about it.

Milke assumes that if a set of Athapaskan cognates has representatives in all three, or even two, of the widely separated areas in which Athapaskan languages are spoken, it goes back to a proto-Athapaskan etymon. From Hoijer's data for the test lists, he compiles a list of cognate sets that meet this criterion. There are 108 such sets out of the total of 153 cognate sets discoverable from the lists. (130 entries on the Athapaskan lists are singular, that is, no cognate appears on another list as the entry for the same meaning.) This list of 108 sets is basic to the rest of Milke's analysis.

Each of the daughter languages represented by a list in Hoijer's data is compared to the list of 108 sets. (Although no etymon is in fact reconstructed for any of the sets, it is fair to treat the sets as surrogates for such proto-Athapaskan reconstructions, if the distributional basis for inferring their inherited status is just.) Finding that the daughter languages differ significantly in the percentage of sets for which their test lists show representatives, Milke infers that each language has its individual retention rate. (The actual range is 70% [Mattole] to 89% [Chiricahua, Chipewyan, Beaver].) He then employs the list of 108 sets to determine "corrected" retention rates for each language, and employs these rates to arrive at novel conclusions about time depths and internal relationship.

There are a number of difficulties with the procedure followed by Milke, inconsistencies both internal and with linguistic methods. An annotated translation of the article is being prepared by Virginia D. Hymes. Here we will stress only that the list that is basic to the procedure is inappropriate and biased.

To speak of retention rates in a glottochronologic sense implies that languages are being compared with respect to their equivalents for the same list. The proto-Athapaskan list to which Milke compares the glottochronologic test lists of the daughter languages, however, is an *ad hoc* and unique collection. It is in no way the proto-Athapaskan counterpart of a test list. This is clear simply from the number of items it contains, 108. The number of items such a list would contain in any language family, following Milke's procedure, might be less than 100 or more. In any case, it would not depend upon the reconstruction of proto-forms in the meanings of the test list, but upon the accidents of distribution of cognates in non-linguistically defined geographic areas.

Such a list would in general contain more than one supposed proto-equivalent for some test-list items, and very likely none for others. To construct an example: If a Northern and Pacific Coast language shared a cognate entered as equivalent to "foot," and another Northern language shared a different cognate entered as equivalent to "foot" with an Apachean language, both would enter into Milke's list. On the other hand, however compelling the full comparative evidence might be for the reconstruction of a particular etymon as the proto-Athapaskan equivalent of "foot," it would not appear on Milke's list unless its descended forms appeared in the meaning "foot" on two test lists in different geographical areas.

One reason for the unsatisfactory nature of this in Athapaskan is that the "North" is a convenient label, not a linguistic unit. The Northern area includes several independent sub-groups of Athapaskan, equal in status to the entire Apachean group of the "South," and, as Sapir noted long ago (1916) in his essay on time perspective, as distant from each other as any is from Apachean. Indeed, when the full story of Athapaskan is available, the deepest split probably will be found in the farthest north near Alaska. The "Pacific Coast" is also yet uncertain as a linguistic unit. These languages, especially Galice, show considerable independence. (They also show the highest number of singular and one-area forms in Milke's tabulation.) Thus the languages located in these two geographic areas are discriminated against in the selection of cognate sets for inclusion in Milke's list. Only the languages located in the south are not, for it is only in the Southern area that geography and linguistic unity perfectly coincide. All the forms found on lists for Southern (Apachean) languages which could distributionally imply a proto-Athapaskan etymon, in virtue of having a cognate in an independent, that is, non-Apachean, sub-group, are likely to be counted, for any sub-group that is independent vis-à-vis Apachean is also in another area. Here Milke's criterion of recurrence in another area coincides with the true linguistic criterion of recurrence in another independent sub-group. But forms found on lists for Northern languages which also would imply a proto-Athapaskan etymon in strictly comparable linguistic terms, because the languages are quite as independent and distant as any Northern language is from an Apachean language, will not be counted, if not also represented in the South or Pacific Coast. It is thus not surprising that the Apachean languages in general show the highest percentages of retention for etymons implied by Milke's list. This fact, which enters into all the subsequent work of the paper, is a result of bias in the method, not of historical reality.

In short, the list for proto-Athapaskan used by Milke seems to be valid neither within the limited context of glottochronology nor within the more general framework of historical linguistics. His conclusions about Athapaskan time depths and internal relationships are in significant part an artifact of linguistically inadequate geographical criteria.

Fernandez (1958) found the same pattern of sub-grouping to result from both the 200-item and 100-item lists, with Mazateco apart from the more closely linked Choco, Popoloca, and Ixcateco, and Popoloca-Mazateco exhibiting the greatest time depth from both lists.

Bright's Hocaltecan results (1956) do not agree in sub-grouping with those of Greenberg and Swadesh (1953). Bright considers the difference due to his use of data superior in quantity and in quality, and to his more objective determination of probable cognates. A check does not show any one of these factors to explain the discrepancies consistently, so perhaps they result from the interplay of all three. The results differ in closeness or distance of particular pairs of languages. Both studies agree in the range of probable cognate percentages found, and in the rather great time depth implied. Bright's study is particularly valuable in highlighting the problem of dealing with imperfect material.

Marshall (1956: 65) states: "Apart from the Tahitian and Tuamotuan figures, Polynesian data show uniform and explainable distortions within a useful range. . . . Variations in results obtained by using differing word lists are clear, but such variations are not only small but are consistently in the same direction and pattern."

We have noted that both Swadesh and Hale find a maximum time depth of 47 centuries within Uto-Aztecan. For the 200-item list, both Swadesh and Hirsch obtain estimates of 1,000 years divergence time within Eskimo. In such cases, when independently obtained divergence times coincide, we can have a good deal of confidence in the result. And when independently obtained divergence times differ, but give the same pattern of sub-grouping, again we can have confidence. But it is clear that different investigators have sometimes obtained results that are not consistent with each other. Such discrepancies might provide valuable insight into the method, historical facts, or both, if we could inspect the test lists used, the material and its sources, and the judgments of cognation made. With these sources of variability uncontrolled, we should not be surprised at inconsistencies, such as those between Gudschinsky and Arana for Mixtecan, Bright and Greenberg and Swadesh for Hokaltecan, Hoijer and Swadesh for Navaho-Chipewyan, or, to be noted later, between Rea on the

one hand and Griffin, Corominas, and Noce on the other for Romance.[12]

In sum, divergence times resulting from the glottochronologic method are not all known to be comparable, since applications of the method have not been made in standardized fashion. Applications made in the same way are comparable, and presumably the divergence times obtained by a particular scholar, such as Swadesh, are generally comparable. Beyond this, we may consider comparable those divergence times in which we have a good deal of confidence, and our degree of confidence must depend upon the circumstances. I should make it clear that I think useful results may be gained even when circumstances are far from ideal. But one has more confidence in divergence times based on known cognates than in those based on probable cognates; more in those based on a number of related languages than in those based on a single pair (like carbon-14 dates in this respect); more in those based on a list used with little or no adaptation than in those based on heavily modified lists; more in those obtained by a specialist in the languages concerned than in those obtained by a non-specialist. These are considerations of method, and I have tried to indicate in the preceding part of this paper the points at which, and the ways in which, divergence times can be affected, as a guide in evaluating a particular result. One must also, as noted, have more confidence in divergence times that confirm and are confirmed by other lines of evidence: "Lexicostatistical data must be coupled with other evidence, including that of archaeology, comparative ethnography, and linguistic paleontology. The separate lines of study serve to verify or correct one another and to fill in details of the story" (Swadesh 1952: 453).

SUB-GROUPING

Within a language family, the patterning of time depths resulting from glottochronology implies a pattern of sub-grouping or internal relationships. Subgrouping has long been a vexed problem in many linguistic fields, but at the same time it is of great importance to anthropology, since it deals with that part of a genetic relationship closest to historical and ethnographic data. The role of glottochronology in subgrouping has been hailed as one of great promise by some, qualified by others, and criticized by still others.

Sub-grouping and Spatial Alignment. Kroeber (1958: 455–56), Driver (personal communication, see now his [6]☆), Voegelin (1958: 50), and others find that glottochronologic data make a valuable contribution to knowledge of internal relationships quite apart from considerations of chronology. Kroeber finds in Rea's Romance statistics, time depth aside, "a neat, coherent, internally consistent classification of Romance in excellent accord with the geographical distribution of the languages tested" (1958: 455). The first model for such a use of cognate percentages is in Swadesh (1950); another kind of model is Hoijer's Table of Athapaskan divergence times (1956*b*: 231, Table VI).

Swadesh has devised a procedure for diagramming internal relationships which makes use of boxes (1959*b*, 1959*c*, 1960). If two languages share more than a certain percentage of glottochronologic sames, they are linked in a chain for inclusion within a box in the diagram. A third language that shares more than this percentage with one of the two is similarly linked to it, and so on until the chain is complete, i.e., until there are no other languages that share more than the requisite percentage of sames with some already established member of the chain. The boxes that contain languages or chains are aligned with each other in terms of successively lower levels of percentages shared by component languages. That is, if a language in Box A shares more than a certain percentage of sames with a language in Box B, these will be linked in the same complex; if not, not. These degrees of relationship are introduced to obtain some qualitative discrimination in what Swadesh views as essentially a continuum of differentiation. (In his view, all native languages of the New World are ultimately genetically related, and the historical problem becomes entirely one of sub-grouping. He approaches the sub-grouping in terms of successive degrees of closeness, not successive splits; in terms of closer or more distant chains, not discrete branchings. See discussion of Hjelmslev, below.)

The major difficulty, pointed out by Swadesh himself, is that the levels of percentages that are used as criteria are largely arbitrary at present. Those in the Oto-Manguean study are *ad hoc* to that problem.

Swadesh (1954*a*) had proposed a correlation of certain terms, for level of genetic relationship, with certain levels of glottochronologically indicated time depth (see also Gudschinsky 1956*b*). These terms—"language," "family," "micro-phylum," "meso-phylum," "macro-phylum"—imply in their hierarchy more of a family-tree model than would seem to fit Swadesh's present approach. Moreover, the chain approach has the result that the extreme members of a chain may differ by more than some members of different chains, so that if found in isolation they would be put in different chains and perhaps in different complexes. (Cf. Hockett [1958], whose concept of the *L*-complex has some analogy to Swadesh's approach.) Thus Swadesh seems to have abandoned any attempt to connect levels of divergence time individually and automatically with levels of family-tree relationship. The direction of his work is toward interpretation of a particular network of relationships very close to the ground. It is still desirable that a large number of cases be examined to see if there are certain percentages of glottochronologic sames which hold up well as boundary markers generally. And it may be that such levels will be found generally appropriate for associating divergence times with such terms as "family" and "phylum," however the underlying pattern of relationships is conceived.

Hjelmslev (1958: 663) attacks glottochronology for basing itself on the unreal conception that, when languages begin to diverge, the separation is sharp and complete. This "family tree" assumption is said to have been abandoned since Johannes Schmidt proposed his "wave theory" in 1872. Note, however, that Bloomfield (1933) recognizes the "ideal type" nature of the family-tree assumption, but justifies it as a necessary heuristic assumption. Lounsbury (1958) uses the family-tree model with full recognition of limitations of the sort discussed by Bloomfield. Gleason (1959) is concerned to establish criteria for choice among the alternative family trees that are theoretically possible for a given num-

ber of related languages. Dyen's theoretical formulations of criteria for inferring migration and for reconstructing meanings presuppose an adequate family tree sub-grouping (1956*b*, 1958). So it is not true that "hierarchical description of a family of languages has been replaced by a topological description" (Hjelmslev 1958: 663). Rather, both are used as complementary perspectives.

Even if true, it would not be the case that "this would deprive glottochronologic method of an essential part of its foundations" (Hjelmslev 1958: 663). We have seen that Swadesh stresses "topological description" in terms of intergrading groups of dialects, making the *mesh principle* a vital part of genetic proof: Indeed, the glottochronologic method is predisposed to favor the mesh principle, or *Wellentheorie,* because of the range of indeterminacy, hence overlap, in the divergence times it obtains, whereas the selected use of qualitative features, such as phonological isoglosses, predisposes toward a family-tree or *Stammbaum* result. In either case, of course, the tendency of the method can be counterbalanced by a judicious user. (Note here Gudschinsky [2]. Her Mazatec studies are a fine example of use of the family-tree and wave-theory conceptions as complementary perspectives. Certainly each must be used for discovery, not obscuration.)

Hale (1958) has difficulty interpreting the internal relationships for the Uto-Aztecan languages. His study has curious features, such as refusing to take advantage of any comparative evidence outside the test-list vocabularies to judge cognates and reconstruct phonology, and not recognizing the greatest divergence time in his Table (47 centuries) as the best estimate for the whole family.[13] Hale uses the tri-partite sub-grouping into Nahuatlan, Shoshonean, and Sonoran, and is troubled that the Shoshonean and Sonoran groups are not equidistant in time from Nahuatlan. This and his discussion of the problem of placing Tarahumara seem to follow from failure to consider the dialects in relation to the map; a variety of geographical alignments could account for the relative distances he finds. More generally, there is a problem only because of failure to use the perspectives of the mesh principle, or wave theory, beside that of the family tree. Lamb (1958: 99), on the other hand, recognizes that "the split of Utaztekan apparently took the form of a gradual spread into a complex dialect area."

Gudschinsky (1955) finds that internal classification of Mazteco dialects by glottochronology and by phonological isoglosses leads to different results; one dialect, San Miguel, causes discrepancy. She concludes that the phonological data give the truer picture, and that "lexico-statistical materials lead to very useful implications as to the historical sequence in which dialect differences developed, but that reborrowing from other dialects following a change in the lines of communication or a shift of cultural and economic dependence, can distort the evidence so that it no longer accurately reflects the true historical development" (1955: 149).

In the particular case, Gudschinsky concludes that San Miguel did not separate from Huautla dialect very recently (as the lexical study suggests), but early, followed by a later *rapprochement* with Huautla (as the phonological study suggests). She postulates this on the basis of documented cases of recent reborrowing, the geographical proximity of the villages, the economic and political dependence of San Miguel on Huautla, and the cultural dominance of Huautla. But note that all the cases are within a millenium or less, and thus at the lower tip of the applicability of the glottochronologic method; more than half the comparisons (eight out of 15 pairs) have divergence equivalent to less than 500 years, and the method is not expected to give results in that range (cf. Hockett 1958: 534). This is also the range within which dialects will most resemble each other, and thus be most susceptible to inter-influence. It does not seem fair to conclude from a study so largely within this range that "any hope of determining absolute time—by some improvement in the precision of lexico-statistical technique—seems to have been permanently lost" (Gudschinsky 1955: 149; also [4]☆). Moreover, phonological isoglosses are not immune to the difficulties found with lexical material. Gudschinsky assumes (p. 146) that the more widespread changes in these closely related dialects occurred earlier than the more localized sound changes that are peculiar to one area. This may well be the Mazatec case, but, in general, as Lounsbury (1958) points out, "An old dialect cleavage may attain a certain magnitude, only to be arrested and superseded by a new one based upon different linguistic features and corresponding to a different geographical split. The later one may grow to proportions which dwarf the overall significance of the earlier split. . . . The widest and therefore most effective splits . . . are not of necessity the deepest ones in time." [14]

The strength of Gudschinsky's particular conclusion lies very much in the geographic and cultural evidence brought to bear on the linguistic results. Thus, see now her more detailed study using the criterion of shared innovations; "the two analyses are basically similar; the difference is one of detail and supporting evidence" (Gudschinsky 1958: 481). In sum, in establishing internal relationships, as in so many other historical activities, one uses all there is to use. The best conclusion is the most probable one in the light of all the evidence.

Greenberg concludes a discussion of sub-grouping with this view: "In so far as the data and state of historical knowledge allow, all types of evidence should be considered. The results in all these domains will necessarily agree if the evidence examined is relevant and due weight is accorded each item" (1957: 55). In this regard, it is important to note that Gleason and Welmers found the family-tree sub-groupings for several African languages to agree when arrived at by three different methods—glottochronology, and two methods developed by Gleason and Welmers, counterindications, and characteristic vocabulary index. Glottochronologic divergence times for Popolocan imply that Mazatec is more distantly related to Choco, Popoloca, and Ixcateco than any of the latter three is to any other (Fernandez 1956), and the same result is reached by phonological reconstruction, counting either number of similarities (Fernandez 1951) or shared structural innovations by addition or replacement (Hamp 1958). Turning to the Pacific area, Eggan (1954) notes a Philippines study for which, since only part of the test-list items were avail-

able, materials at hand were used to develop a table of relative relationships that gave important information. Dyen (1953*b*) had been working independently on central Philippine languages, using both shared innovations and lexicostatistic techniques; there was enough overlap in the groups studied to discover the degree of correspondence between the two sets of results, and this was promising enough to launch a further program.

Dyen himself has reviewed Dahl's attempt to determine the closest Oceanic relative of the Austronesian languages spoken in Madagascar, and found glottochronologic results to agree with Dahl's conclusion, based on all available linguistic evidence of other sorts, that the last unity of Malagasy was with Maanyan of southeastern Borneo. Dyen observes that "the procedure of subgrouping by lexico-statistic dating is the only one available where 'inspection' or the search for a shared innovation yields no result. I had found Dahl's hypothesis wholly dubious until I submitted the languages to lexico-statistic dating." Dyen concludes: "From the linguistic point of view this book is interesting because it attempts to handle the problem of subgrouping in more or less the traditional manner. Some may believe Dahl proves his point; but the procedure leaves wide room for controversy because it cannot calculate the chance factor. The agreement with the results from lexico-statistic dating apparently removes all doubt, provided indeed that that procedure itself is reliable" (1953*a*: 590).

Grace has published a detailed study of sub-grouping within Austronesian (Malayo-Polynesian), concerned with the position of Polynesian. In his Appendix I he presents calculations for both the 200-word and 100-word lists. Two calculations were made for each list, one excluding and one including probable cognates and probable non-cognates. In all the calculations, only one figure failed to confirm positively the hypothesis of closest relationship between Maori and Tongan (of the groups compared), and that Maori, Fijian, and Rotuman are more closely related to one another than they are to Mota. This one figure in calculation 2, with the 200-item list, did not discriminate the Fijian-Mota relationship from those of Maori, Fijian, and Rotuman with each other. Grace concludes: "That figure, however, is not sufficiently at variance with the pattern which holds elsewhere to suggest any alternative hypothesis. The hypothesis suggested by the glottochronological counts clearly agrees with the hypothesis with which the body of this study is concerned" (1959: 69).

In the body of his excellent study, Grace uses the method of exclusively or uniquely shared innovations which was explicated by Brugmann in 1884. This method is decisive when such innovations can be determined, but as Dyen points out above and in [1],☆ the method is quite laborious and yet cannot be certain of reaching a determination. Moreover, since glottochronology relies on the percentage of shared cognates (simply translating these into time), its value is essentially whatever value lies in the use of basic vocabulary. Dyen (1959: 549–50) suggests that this value is high, and I would agree. It is not displaced by the theoretically precise, but practically often inconclusive, criterion of shared innovations.

In sum, the test-list vocabularies are valuable objective evidence of degree of relationship. They should be correlated with any other lines of evidence available—linguistic, cultural, and geographical—in arriving at a final determination. As phrased by Swadesh (1960):

> No transcendent reliability is claimed for lexicostatistics, either in general or in the special form which we have used. Is is considered to be one of several possible indices of affinity, which normally coordinates well with other lines of evidence. It has the advantage of fairly easy application. . . . However, in every case in which other information is available, it must be taken into account to determine whether it corroborates or changes the results. In the event of contradictory indications in two kinds of evidence, we must not assume any one to be necessarily superior but examine each concrete instance on its merits.

When a determination has been made, glottochronology provides an estimate of time depth usually not obtainable in any other way.

Sub-grouping, Language Limits, and Individuals. In most applications of glottochronology and lexicostatistics, it is taken for granted that the lists represent distinct languages or dialects. If the list is obtained from one individual, it is treated as representative of a dialect or language to which the speech of that individual belongs, not as representative of the individual in and of himself. Yet the question of sub-grouping, of placing non-identical sets of speech habits in relation to each other, can be theoretically extended past dialect differences to individual differences. Indeed, this perspective underlies the treatment of dialect and language boundaries by Hockett (1958) in terms of idiolects, and is stated by Dyen [1].☆

This is an empirical matter in glottochronology, for members of the same speech community may supply test lists that are not identical. In terms of a formula for application, this might be taken to imply some amount of divergence time between residents of the same village or even members of the same family. Clearly there is need to consider the problem of a lower limit for the historical validity of glottochronologic time depths. We have stated before that the method does not apply when divergence times of 500 years or less result. Although Gleason,☆ for one, sees no reason for a sharp cut-off point, and suspects that to define one is to attempt to read out embarrassing cases, I think it is clear that the question of a limit is relevant, and that, as Gudschinsky [3]☆ states, it leads to the fundamental question of what it is that glottochronologic divergence times measure.

Gudschinsky's suggestion of a difference of nearly 600 years in an almost completely homogeneous linguistic community is a hypothetical construction. Empirical studies made by Norman McQuown in the Mayan Tzeltal and Tzotzil villages of Chiapas, Mexico (and also by Satterthwaite with respect to Arabic), show a maximum variation within a community approaching 10%, equivalent to about three minimum centuries of divergence (Swadesh☆).

We greatly need further studies of intra-community variation with respect to basic vocabulary, particularly test-list equivalents. Presumably no one would assert that dialects are perfectly homogeneous. Clearly a community can maintain a certain degree of divergence in all aspects of language—phonology, grammar, and lexicon—for an indefinite period, as it develops through

time. This must be analyzed both in terms of the linguistic code and in relation to the social, cultural, and personality systems involved. We must assume such intra-community variation in both the daughter languages and the parent language whose time depth we seek to estimate. Further studies can help establish the limits of such variation. The upper limit, if a general one appears, will be the lexicostatistic language limit described by Dyen,[☆] the boundary below which time depths are meaningless.

The fundamental theoretical question, to which no adequate answer can be given here, is this: Does the glottochronologic method measure one thing below such a limit and another above it, that is, intra-dialectal variation versus time depth? Or is there a sense in which it measures the same thing, but a thing that enters into a meaningful relationship with time only above the limit? I suspect that the latter is the case—that the appropriate view is that the test lists measure divergence, degree of separation, and that this degree of separation may exist up to a certain point in dependence only upon factors within a community, but that a degree of separation beyond that point obtains only between communities. In effect, glottochronology does measure the end of effectual communication, in that range in which it can meaningfully deal with time. Underlying degree of separation is both space and time, but as the factor of space increases quantitatively, it reaches a point of qualitative change, that point at which any increase in spatial separation is simply more of the same thing, namely, sufficient space for an effectual end of communication. (Of course the factor of space is partly a matter of geography, partly a matter of social definition.) This point of qualitative change is the limit at which the appropriate measurement of time begins.

FIT WITH OTHER EVIDENCE

A survey of applications of glottochronology affords a check on the goodness of fit between glottochronologic results and other lines of historical evidence. It has been pointed out that this is an important check on the validity of glottochronology itself.

Hattori's striking confirmation of the retention rate in his study of Japanese dialects has been cited. Of his historical interpretation of the results, after the introduction of his correction for underestimation, Hattori states: "The assumption does not seem to contradict the historical and archeological evidence" (1953: 77).

Taylor's results for two Island Carib dialects fitted with historical facts and with the rate of retention (Taylor 1952: 229).

Taylor and Rouse (1955) discussed the date of separation of Island Carib (Dominica) and Lokono (British Guiana), two Arawakan languages. Essentially, there was a provisional fit of the glottochronologic and archaeologic data on two counts, but a fit dependent on the outcome of further archaeologic dating and historical interpretation in the area. Subsequently obtained radiocarbon-14 dates fit neither this linguistic dating nor the rest of the archaeological evidence. Swadesh (1958: 673) gives a new divergence time of 24 minimum centuries, longer by six than that considered by Taylor and Rouse. Its fit with other evidence has not been publicly evaluated.

Raun presented glottochronologic results for Finno-Ugric languages to the Ethnolinguistic Seminar at Indiana University in the academic year 1953/54 (MS.); he considered their conformity to other lines of evidence to be quite satisfactory.

Hencken has compared Swadesh's Indo-European estimates with the archaeological picture. The first estimate of English-German divergence was recognized as too recent, but a more detailed study (Arndt 1955) gives a longer divergence time that fits the historical record more closely. Regarding the Slavonic-Teutonic split, Hencken concludes: "Hence linguistic paleontology in this case leaves us with the same two choices (because of the range of possible error) as does archaeology, though without telling us which to choose. But it may not be too much to hope that this or some other objective method may be further developed to supply additional evidence" (1955: 57–58).

Elbert (1953) finds that his Polynesian results check with other information about Hawaiian prehistory. Marshall (1956) criticizes Elbert's results, but presents results from his own application of the test list and his modified list, which he uses in an interpretation of Polynesian prehistory.

Ravicz and Romney (MS.) have used tentative divergence times for Mixtecan in order to place the time depth of their reconstruction of proto-Mixtec kinship. They found the results consistent with other knowledge of Mixtec. The first divergence times computed by Swadesh from partial data gave 16 centuries; around 15 minimum centuries divergence time has since been reported (Fernandez, Swadesh, and Weitlaner 1959: 56).

Using the 200-item list but with incomplete data, Lounsbury has calculated an Iroquois-Cherokee divergence of about 3,500–3,800 years, "a trifle shorter than my first guess" (about 4,000 years), "based on a rough subjective evaluation of the amount of phonetic, grammatical, and lexical change which has accrued to Cherokee and which sets it off from the rest of Iroquoian" (1958). His revised scale (Lounsbury 1957) was used.

The Athapaskan results (Hoijer 1956*b*; Hymes 1957*b*) fit well with other knowledge of these peoples; the time depths calculated with the 86% retention rate for the 100-item list (Hymes 1957*b*) are on average 300 years longer than those first obtained, and may fit more easily with archaeology and ethnology. Since all Athapaskan divergence times fall within the last two millenia, Swadesh's rough estimate of the Tlingit-Athapaskan time depth as more than two millenia is appropriate (cf. Swadesh 1958: 14, 21; Kroeber 1955: 92, n. 3).

A number of New World results have been surveyed by Swadesh (1954*b*), Kroeber (1955), and Eggan (1958). In the first survey, Quimby finds no archaeological evidence yet available to correlate with Chitimacha and Atakapa divergence times. Haury finds the Utaztekan time depths of thirty or so centuries plausible, and that the glottochronologic and archaeologic time estimates for Uto-Aztecan divergences support each other and may well be related within broad limits. The present

estimate of 47 centuries maximum divergence has not been evaluated archaeologically. Ekholm finds the one pair of Mayan languages for which archaeological reference is possible to be not far off in glottochronologic estimate from the estimate of archaeological reconstruction, though he would have guessed the separation of Huastec from the main body of the Maya to be later than Swadesh's date.

Collins finds the 3,000-year estimate for Eskimo and Aleutian in close agreement with the evidence of archaeology, dendrochronology, and radiocarbon analysis. We have noted that recalculations with the 100-item list (by Swadesh and Bergsland) both gave considerably greater time depth (Swadesh: 46 minimum centuries divergence); how this might fit with the archaeology of the area has not been stated in print. In any event, Eggan (1958: 648) comments that "a mere comparison of Eskimo vocabularies would have made many of the recent controversies as to Eskimo origins pointless," and Kroeber (1955: 92) finds the fact that the two Eskimo languages show the most recent separation reported in Swadesh (1954*b*) "as it should be, for the close relationship of Alaskan Eskimo to Greenlandic has been evident ever since southwestern Eskimo vocabularies were available."

Regarding some South American estimates and resulting classifications, Eggan (1958: 650) observes: "Here the data available are generally poorer than for North America, but the regroupings sound reasonable in view of the archaeological and ethnological data and the parallel situation in North America."

Kroeber (1955) rules some cases out of consideration as methodologically impure, accepting the rest, and comparing implications of the time depths to earlier results of "inspectional" comparison and to geographical distributions. For the most part, he finds the fit good; where it is not so, he sees need for finer analysis. Taking a particular pair of glottochronologic results, Kroeber turns discrepancy with previous notions into a source of new insight. He examines the glottochronologic conclusion that the time depth of Salishan is greater than that of any other Powell stock so far tested, a depth like that within "super-stocks" or phyla such as Penutian, Hokanoid, and Huave-Mayan. The explanation is that fraction of retentions may agree, but sources of replacements differ. The largely adjacent Salishan languages are freighted with replacements by shared innovations, and borrowings from one another. The largely separated Hokan languages are interlarded with unique innovations and replacements by borrowing from non-Hokan languages. Kroeber concludes (1955: 103):

> These are specific historical reconstructive adumbrations. They do show what vistas are opened up as soon as linguistic distributions on the map are added to linguistic time depths. They also show that time computations must be read with reference to the map, not *in vacuo*. The much greater time depths of the Salish than of the Hokan languages would be misconstruable in the abstract, as indicative of less proneness to immanent or spontaneous change, in view of the fact that Salish languages ever since first known have been accepted as a genetic unit whereas the Hokan ones were long not recognized as affiliated with one another at all. In fact the time depth findings might be misleading if they had not been brought into relation with Salish intergradation and territorial continuity and Hokan linguistic and geographic discontinuity.

We have noted the report that the test lists show time depths of only a few hundred years between southwestern Norwegian and Icelandic, "whereas, as is well known, the separation took place about thousand [*sic*] years ago," and some 1,000 or 1,300 years for Modern Georgian and Modern Mingrelian, "whereas the separation can be shown, by usual [*sic*] historical methods, to have taken place more than thousand years earlier (Vogt, 1942, pp. 183ff.)" (Bergsland 1958: 655).

The most notable discrepancy between glottochronologic estimates and other evidence is reported by Rea (1958) . Rea compares eight Romance languages on the 100-item test list, calculating fractions of common cognates, and divergence times. The divergence times turn out to be markedly too low.[15]

Rea's claim to have disproved that glottochronology has "any usefulness or validity even for the languages upon which it is based" has come under attack by Taylor (1959), Householder (1958), and Kroeber (1958). Taylor points out that Rea "translates" no less than twelve of the items on the list for which Spanish glosses are provided by other, unrelated Spanish words; Taylor doubts that native speakers would consider these "common, everyday equivalents." He also criticizes some French equivalents, and concludes "enough has been said to show that Rea was careless of the meanings required or of their 'common equivalents' or both; and a tool should not be condemned by one who has not yet learnt to use it properly" (1959: 71).

As to sub-grouping, Kroeber averages the retention rates for each of the eight languages, and concludes "what the statistics result in is a neat, coherent, internally consistent classification of Romance in excellent accord with the geographical distribution of the languages tested" (1958: 455).

As to the too short divergence times, Householder (1958: 402) points out that the proper method would be to compare the Romance languages individually with Latin (or with their reconstructed parent, proto-Romance) (we have mentioned earlier that such a procedure [strongly advocated by Marshall (1956)] avoids many difficulties of underestimation); or to use the modified formula $t = \log C / \bar{s} \log r^2$, or interpret t as a *minimum* divergence time. "Practically all of Rea's argument is dependent on this error." Householder also finds errors in the choice of equivalents (e.g., French *ne* for 'not' instead of *pas* [agreeing with Taylor]; *que* for 'what') and refers to "some rather preposterous statements about the true divergence time being 100 A.D. or 250 B.C."

Kroeber multiplies Rea's dates by a J (jiggle factor) of 2.2 to get divergence dates that are in line with Rea's expected dates, and that also show some interesting accord with history (e.g., the conquest of Dacia). It is not necessary, however, to consider the estimated time depths so greatly in error. Though Rea writes of the greatest divergence time as that between Sardinian and Rheto-Romance in the ninth century A.D., his table shows Rumanian-Spanish the greatest—1,082 years divergence or A.D. 874. But correction of Spanish entries in accord with Taylor gives a net reduction in the number of shared cognates of three, hence a cognate percent

of 68%, a time depth of 1,278 years, and a date of A.D. 678. If Hattori's *J* of 1.4 is used, the resulting time depth is 1,789 years, and the date is A.D. 167. Even in Rea's terms, especially in the light of Householder's comment, this can hardly be said to depart seriously from the likely true date. The usual procedure of taking the greatest divergence time as the best estimate for the whole family would find this a reasonable result.

The other main application of the method in detail to a well-attested language family is that of Arndt, for Germanic (1955, 1959). Arndt established a rectified retention rate for English, finding the gross rate 67.8% per millenium, and the rectified rate, allowing for inter-dialect borrowing, as 74.8% (all this with the 200-item list). With care for such problems as short-run distortion, dialect re-centering, and literacy, Arndt obtained results that seem to have come out remarkably well, reinforcing current doubts about the conventional picture of the development of Germanic, and supporting opinions based on recent archaeological and dialect-geographical work.[16]

In general, discrepancies may enlighten us. This has been the case with the Athapaskan calculations, and with the Salishan-Hokan time depths analyzed by Kroeber. (Kroeber recommends the same approach to the discrepancies between the results of Rea and of Lees's collaborators [1958: 456].) Swadesh has concluded (1955*b*: 123–24):

> There is already sufficient evidence that lexicostatistically estimated time depths are to some degree approximately related to actual time. This is shown by a number of correlations, including several historical dates and a few archaeological ones, including some obtained by the carbon method. And there is further evidence of general correctness in the inner consistency of lexicostatistic dates found among the languages of a family or stock. Without claiming for lexicostatistics the accuracy of a precision instrument, there can nevertheless be no doubt that it has considerable approximate validity and is already capable of serving as a helpful tool for the reconstruction of prehistory.

The preceding survey of applications largely warrants this conclusion. The vast bulk of the cases are found to fit external evidence, or not to violate it. On the other hand, we must remember that in many cases, especially those of prehistory, no very certain association between a date of linguistic separation and other evidence is possible. Moreover, if we have confidence in the glottochronologic approach, then we will not expect exact or dramatic fits between its estimates and dating from other sources, but accept approximate fits as about as reasonable a match between the streams of linguistic and cultural change as one can expect, and regard serious discrepancies as problems for closer historical investigation, rather than reflections on the method.

I must regard the utility of the method as supported by the applications so far. Not the least utility is the heuristic value of revealing exceptions, the further study of which brings further insight. But the presence of such exceptions reminds us that the method is still in a developmental stage, and that a major obstacle to evaluating applications, and to further development, is the failure of most studies to make public their supporting data.

USES OF LEXICOSTATISTICS

IN THE INTRODUCTION, *lexicostatistics* was defined as a broader field of which glottochronology is a part, and, except where quotation has prevented it, the terms have been so distinguished. Here we will take up three aspects of lexicostatistics: sub-grouping; proof of genetic relationship; and rates of lexical change.

SUB-GROUPING

Something has been said of this under applications of glottochronology. We should note here that counting common vocabulary is an obvious and old device for sub-grouping, but its rigorous formulation is not. In a recent symposium on genetic relationship among languages, Gleason (1959: 23) issued a call for further development of lexicostatistic methods for ascertaining internal relationships. These serve both in reconnaissance work, giving provisional results of some validity and a basis for planning the use of more complex methods, and also in formulating a genetic tree or *Stammbaum*. "Lexico-statistical methods seem clearly the most feasible, perhaps the only feasible, methods of accomplishing either end at any but the most advanced stages of historical reconstruction." Gleason demonstrates two such methods. That of *counterindications* calculates the number of word-sets that give evidence that a pair of languages are *not* immediately related, that is, not more closely related to each other than either is to any other language under scrutiny. The method requires not less than four languages. When the total counter-indications for all pairs of languages are examined, a clear minimum for two languages implies that they *are* immediately related. The method of *characteristic vocabulary index* (tentatively so named) gives a credit of one for each cognate set, dividing the credit equally among all pairs of languages participating in it. When the totals of credits for all pairs of languages are examined, a clear maximum indicates immediate relationship.

The value of such methods is both general and particular. In general, it is valuable to have an array of methods, since one may give a clear indication where another fails. In particular, these methods can sometimes be used with vocabularies smaller than the glottochronologic test list, and they do not require that the available vocabularies be basic in nature.

PROOF OF GENETIC RELATIONSHIP

We must reiterate here that to estimate divergence times within a group of related languages is one activity, while to use the same vocabulary materials to explore or prove relationship is quite another. The basis of such lexicostatistic work is, however, an extension of the probabilistic basis of all proof of genetic relationship. As Greenberg, Swadesh, Gleason, and others have recently stressed, and as Bloomfield, Meillet, and others had made clear before, the essential requirement is to adduce resemblances that can be reasonably explained only as due to retention from a common ancestor—that is, to show resemblances among a set of

languages that are, first, not due to chance, and, second, not due to diffusion.

Gudschinsky [1]* questions the notion that all evidence of relationship is primarily statistical. My views are developed in Hymes (1959*a*). Here I would say, first, that if "instances of greater than chance resemblance are assumed to imply genetic relationship," I do not see why they cannot be "proof" of it, and, second, that I understand "statistical" in a general sense, such that if it is necessary to eliminate chance as an explanation (as is the case in showing genetic relationship), then one's evaluation of any evidence is implicitly or explicitly statistical in nature. With Gudschinsky I attach importance to a small body of patterned similarity, following Sapir and Hoijer in this regard. But I take the point to be that such patterning makes chance (often also diffusion) unlikely as an explanation of its presence in separate languages.

In cases such as Athapaskan, Algonquian, or parts of Indo-European, the quantity of resemblances, and the complexity of the systematic inter-relationships, make genetic connection almost self-evident. Strictly speaking, there is so much evidence conforming to our expectations that we do not demand a statistical proof that it is greater than chance, although statements such as "neither accident nor borrowing will explain them" (Bloomfield 1933: 299) imply the possibility of such a proof. Greenberg (1953, 1956: 46–55) gives renewed attention to the problem with regard especially to basic vocabulary, and, during the past decade, Swadesh has tried to develop explicit, quantitative criteria for deciding whether resemblances are due to chance or to historical connection (see particularly Swadesh 1951*a*, 1956*d* [prepared 1952], 1953*a*, 1954*a*, 1955*c*, 1956*a*, and 1959*a*; see also Kroeber and Chretien 1937, 1939; Chretien 1943, 1956; Collinder 1948; Greenberg 1951 and argument in Hymes 1959*a*).

This development is radical only in the light of insistence on some one of the several possible lines of evidence for genetic relationship. In one of his earliest papers on the subject, Swadesh assessed the fact that, considering only the intransitive subject in the independent declarative mode, ten of Powell's 57 North American language families have both *n* in the first person and *m* in the second person, far exceeding his calculation of the chance expectation. He notes that seven of these ten are in Sapir's Penutian, and that the other three suggest an even deeper grouping involving Azteco-Tanoan and Hokan-Coahuiltecan (a suggestion that Whorf and Trager have also put forward). In this regard, it is striking that Meillet begins an exposition of the method of comparative grammar with an argument based on the first- and second-person pronouns in Romance. While warning that the totality of facts is very complex (like Swadesh 1956*d*: 383), Meillet states, "The essential point of the reasoning is this: the concordance of Romanian *eo*, Italian *io*, Old French *jo*, Spanish *yo* can not be chance" (1958: 21). Meillet goes on to consider "un calcul des probabilités" what chance there is for a given meaning to be expressed by a given combination of sounds, only to reject attempting one on the grounds that the intervening conditions are too complex, and to conclude again: "Without recourse to calculation, which plays no part here, one can justly affirm that a concurrence of four languages in the expression of the first person by forms so similar, of the type *yo*, can hardly be taken for an accident. And if, as happens in this case, a whole series of other languages present analogous forms: *eu* in Romanian [*sic*], *iou* in Rheto-Romance, etc., chance becomes more improbable" (1958: 22). (He continues that chance is still more excluded when one observes the other pronouns.)

If anthropologists were to accept the distinguished comparativist's reasoning here, the reality of Sapir's Penutian and even of Whorf and Trager's Macro-Penutian could hardly be questioned a moment longer.[17] It remains, however, that one's intuitions of chance expectation may not coincide with those of other scholars. Despite Meillet, an explicit procedure for calculating chance expectation is needed. If the task is complex, its difficulty only adds to the merit of those who undertake it.

Such work is valuable particularly in the areas of the world whose genetic relationships most concern anthropologists. Much still remains to be done there, and much of the material can be evaluated only by such means. And in exploring and demonstrating long-range connections, for which the quantity of inter-relationships is inevitably small, such work is a fundamental advance.[18]

Two essential qualifications must still be made. First, if five, six, or seven lexical resemblances between two languages are greater than would be expected by chance, even at the 1% level (Swadesh uses the 5% level), this means simply that only in one, out of 100 cases such as this, would chance explain the similarity. Other considerations might still lead us to take this case as that one. First, when we accept or reject the chance interpretation, we do so partly on the basis of non-statistical evidence, such as geographical distribution. Second, the statistical demonstration does not in itself discriminate between resemblances due to borrowing and resemblances due to genetic relationship. It is necessary to consider the character of the lexical items themselves, and, again, whatever else is known about the languages and the peoples speaking them.

Swadesh makes clear that he eliminates from consideration those vocabulary items likely to be similar because of onomatopoeia and sound symbolism. Recent borrowings, say from Spanish into an American Indian language, are ordinarily obvious. It still remains that to show the probability of historical connection does not show whether the connection is based on borrowing or genetic residue (Swadesh 1951*a*: 1). Here great reliance is placed upon the postulated resistance of basic vocabulary to borrowing (Swadesh 1954*a*: 313, 314). It is true that post-contact borrowings into the basic vocabulary of New World languages are amazingly few, and that this may well indicate an extremely low incidence of borrowing into basic vocabulary in pre-Columbian times. Sometimes the geographical separation of languages may rule out borrowing, or place it at such an ancient date as to make this kind of historical connection itself of great significance. Sometimes the quantity of lexical items that show resemblance is greater than any reasonable estimate of diffusional cumulation (although this estimate needs careful revision, since first proposed in Swadesh 1951*a*: 16). And we may recall that

Meillet (1958: 59), despite his dictum that vocabulary is the domain of borrowing, also recognized a basic sector that was largely conserved.

Still, when the quantity of lexical items showing resemblance is only slightly above chance level, this is not ineluctable proof of genetic relationship. The logic of historical linguistics does not permit us to reduce the problem "to a choice between the factor of pure chance and the effect of common origin" (Swadesh 1954*a*: 315). Since borrowing into the basic vocabulary does occur, though slight, and since diffusional cumulation does occur, though small, the shadow of diffusion still hovers over any case dealing with quantities as small as 5% of a 100-item list. It may be eliminated through qualitative considerations, such as phonological correspondences, geographical distribution, and so forth, but not by statistics.

I must state that I will be surprised if most of the distant connections proposed by Swadesh are not ultimately confirmed (cf. Hymes 1956*b*: 595; 1957*c*: 79ff.) Let us again cite Meillet. He maintained, "It is never by differences or concordances of vocabulary that one can establish relationships of languages" (1958: 46). But he also pointed out, regarding the use of vocabulary to probe relationship, "But the normal continuity of vocabulary in languages having the same point of departure shows that it is necessary not to neglect it. It is the concordance of *father, mother, brother, sister* which most suggested the idea of the initial community of the Indo-European languages" (1958: 46). And, after again deprecating the use of vocabulary to prove relationship, he said: "It remains moreover true that, in most cases, the common words of the vocabulary remain in large part the old traditional words. In a language as profoundly altered in all respects as English, the examination of the pronouns *me, we, us, you*, of the numerals *one, two, three, ten*, of the kinship terms *father, mother, brother, sister, son, daughter*, of the verbs *be* (*is, was*), *come, eat, love, bear*, etc., would suffice to proclaim the Indo-European character of English" (1958: 108; cf. also p. 94).

The great variety of the world's languages must reduce ultimately to a small number of distant, far-flung genetic relationships. In gradual penetration of these reaches, we proceed by stages of hypothesis, proof, and establishment. Hypotheses provide guides for further research; proof shows that no other explanation than that of genetic relationship is tenable for a body of resemblances; establishment unravels the details of relationship as fully as possible. As we proceed from the first to the third of these stages, the value to historical anthropology increases. The confidence it may give, and the information it may receive, are greatest when extensive sound correspondences and etymologies have been established at the third stage. We must start, however, with the first stage.

Here lexicostatistics stands in the role of a pioneer in probing and sometimes proving the distant relationships that lie (some perhaps forever) beyond the full refinement of comparative methods such as sound correspondences. To quote two early exponents of a lexicostatistic approach, "What counts in a question such as this is not the thousand points that can not yet be construed as evidence, but the hundred, or failing those the ten, that can" (Dixon and Kroeber 1919: 113). It would be mistaken "to insist that no judgment can be considered that is not substantiated by evidence of the quantity, and of the degree of accuracy, that Brugmann would demand. Such an attitude, if persisted in, cannot but result in complete barrenness of effort for a long time to come. American linguistics is a new and undeveloped field, and pioneer conditions must continue to prevail in it for some time longer. To throw out all evidence that does not attain to an arbitrary standard of fineness is sterile as well as unreasonable" (Dixon and Kroeber 1919: 113).

It has been asked, "Can a test list of but one hundred items possibly be adequate for exploring time depths of considerable antiquity?" It should be clear that I believe the answer to be "Yes." A second query would be, can such a list provide proof of genetic relationships of considerable antiquity, and here the answer must be, often not. Other lexicostatistic lists are possible of course. The direct comparability of the test-list items, and the relative ease with which comparable material can be obtained, make glottochronologic test lists one especially useful tool of exploration.

RATES OF LEXICAL CHANGE

Glottochronology has been to the fore in recent lexicostatistic work, and the desire to estimate time depths leads to a focus on that part of vocabulary which seems most stable. Noting this, Swadesh (1955*b*: 122) observes that "only a fraction of the total phenomena have been studied. Eventually it will be desirable to study rates of change in the various types and levels of less stable vocabulary and to develop a complete theory of the factors affecting the rate of vocabulary change."

Even severe critics welcome this goal (Bergsland 1958: 657). Eggan observes that even if retention rates should be found to vary with language family or sociocultural conditions, the establishment of such limited constants would be a worthwhile advance, and he points to areas, such as the Mountain Province in the Philippines, where it may be possible to study the processes involved in reasonably direct fashion (Eggan 1958: 651–52).[19]

We have noted problems of interest within basic vocabulary itself, regarding the relative stability of semantic groups within a language family and across language families. The same problem of course is of interest throughout vocabulary.

Where some independent measure of time depth can be obtained, either from other historical data, or by comparing daughter languages individually with a reconstructed parent, it will be important to pursue the implications of $\bar{s}$, average degree of separation, as it enters into calculations of divergence time from comparisons of daughter languages among themselves. Put otherwise, it may be that the *J*'s (jiggle factors) required by such comparisons will have worthwhile implications for the study of past inter-influence among languages, the effects of drift, supposed conservatism, and the like.

Here lexicostatistics must contribute to and draw from the general theory of culture change.

THE STRATEGY OF LEXICOSTATISTICS

THIS STUDY has found for glottochronology and lexicostatistics neither an elegant argument nor a simple recipe. Rather, it has found a field of inquiry bristling with scattered results and partly answered questions. This is not too surprising, given the short history and few students of the subject.

We have found further research and application justified by results so far. There is support for the concept of basic vocabulary; some tendency for regular rate of change in the part sampled by test lists; and some consistency of estimates of divergence time with other lines of evidence.

The history of science would seem to show that any such empirical regularity should be probed for all it is worth. Even if any one retention rate should not prove universally valid (or useful), the close agreement of Dravidian Kannada, Japanese, Arabic, etc., would require explanation. The burden is not that of "glottochronologists" alone, but of all students of language. Consider some hypotheses: (*a*) there are no universals in human experience for which all human languages provide a means of reference; (*b*) there are no possible general rules for selecting equivalents to a universal list of meanings from among the semantic patterns of different languages; (*c*) there are no items of vocabulary which are relatively more frequent, persistent, and universally translatable than others; (*d*) for any set of items of vocabulary within a language, the rates of change distribute randomly; (*e*) for any set of items of vocabulary, the rates of change in different languages distribute randomly. If we question the hypotheses of glottochronology, do we accept these? What is the range of relevant hypotheses, and how shall we choose among them? How indeed, except by in effect subjecting glottochronology to the thoroughgoing exploration it needs? (Cf. Gleason [1959: 32] and his three premises cited earlier.) In short, the need of glottochronology is part of the need for general theories of semantic patterning and lexical change.

With regard to the basis of glottochronology, systematic test and experimentation with lists is particularly needed, along lines suggested, such as alternative parts of lists, supplementary items, semantic groups. There has perhaps been a tendency to develop the test list in ways that would facilitate field-work completion and long-range comparison. These are worthy objectives, but other considerations in choice of items should prevail, if supported by control-case studies or specialized knowledge of languages concerned.

The theory of basic vocabulary is inchoate. Here the aid of social psychology should be sought. Note, for example, Brown's comparison of the most frequent items in adult and child vocabularies, indicating that the glottochronologically desired attributes of simple, clear reference more often characterize the most frequent nouns and verbs of children (1958: 249). The concept *basic* is significant for time perspective in all of historical anthropology (Hymes 1957*d*), and a unified theory of cultural change must take close account of the insight that may come from vocabulary, the part of language most sensitive to outside cultural forces.

With regard to applications, present ones are very much approximations, though sometimes very good ones, confirmed by other evidence. Present studies can contribute as much to improvement of method as to historical knowledge. That is, their results can contribute if the minimum requirements specified by Kroeber (1958: 457) are met: both the full list of test-item equivalents, and the judgments of cognation, must be made public. I would add that both diagnostic and supplementary lists should be completed if at all possible; that all questions regarding choice of equivalents should be presented; that the criteria for judging cognation should be given; and that the whole is best based on a specialist's knowledge of morphological structure and semantic detail. Such studies contribute to development of method by making replication and re-analysis possible; any such study can contribute consciously to the topics raised in the section on applications.

The first flush of excitement and application has abated, and glottochronology and lexicostatistics need now a period of painstaking, precise development. Enough has been gained to show the importance and profit of the subject. Enough has been learned also to show that no formal proof now justifies the basis, and no casual application now gives certified results. Careful work and the widest possible application should be the order of the day.

I have tried to show that lexicostatistics is but the intense focusing of interests long present in linguistic work. The concept of basic vocabulary is old, and the statistical nature of genetic proof has long been implicit. It is the calibration with time that is new and most exciting to the anthropological historian, and that must be most carefully handled to avoid premature disillusionment. I have tried to consider the criticisms that have been raised, and have raised many myself, for progress must be founded on frank recognition of points where it is needed. Yet, as I have said, the over-all trend of results so far shows the importance and profit of the subject. This evaluation seems concurred in by some of the most responsible and acute critics of present weaknesses, for they have matched their analysis with substantive applications (Gudschinsky 1955; Hoijer 1956*b*; Lounsbury 1958), or explication of results (Kroeber 1955, 1958). Lexicostatistics is not a short-cut; it does not replace other methods and information, but must be incorporated with them into a consistent body of knowledge. That it is a valuable addition, and that it will increase in value, can, I believe, scarcely be doubted.

The use of lexicostatistics in long-range comparison is important, but the anthropological value of lexicostatistic results will be widest at present in those cases where the results of the comparative method can already be built upon, and where, as Romney (1958) terms it, the genetic model can be employed. Such cases are ones where judgment of cognation leaves least to guesswork; where reconstruction of proto-vocabulary helps control the effects of drift, contact, and dregs; where a language family of not too great time depth provides a framework within which all lines of anthropological evidence can be meaningfully compared and controlled (cf. Vogt 1959). This coincides with the advice of Eggan (1958: 651) and Kroeber (1955: 103–104; 1958: 456).

Of studies made with the revised, 100-item list, Hoijer (1956*b*) is so far the exemplar. It will not have escaped notice how often his has been the only published basis for discussing important questions of method.

The general strategy of lexicostatistics must be sober advance on several fronts. Further research and improvement in the basis of glottochronology will make that method more widely useful and its results more certain. Further applications to known language families, as Kroeber says, "will help to refine the glottochronological method and probably to give us unsuspected new principles and general insight" (1955: 103). They will also provide useful estimates of divergence time. Further studies in lexicostatistics will help refine the quantitative criteria and probabilistic basis for proof of genetic relationship. They will also provide a series of hypotheses and proofs that could perhaps not be obtained in any other way. At present, "there is every reason to believe that lexicostatistics has not yet reached or even approached its maximum potentialities"; we must "hope for the major research which can definitely establish the capabilities and the limitations of the lexicostatistic method" (Swadesh 1955*b*: 124, 131).

Notes

1. Hockett 1953: 151.

2. I offer my thanks to those who have commented on the draft of this paper for correction, stimulation, and occasional kind words (omitted here). In preparation of the draft I have been variously indebted to Fred Eggan, Maria Teresa Fernandez de Miranda, David H. Kelley, Alfred L. Kroeber, Robert B. Longacre, Floyd Lounsbury, Oriel Pi Sunyer, Robert Ravicz, J. David Sapir, Volney Stefflre, William C. Sturtevant, Morris Swadesh, and especially my wife, Virginia. Donald S. Marshall kindly has given permission to quote from his unpublished dissertation. The Laboratory of Social Relations, Harvard University, has supported my research during the preparation of this study.

3. This principle, especially in this context, might be taken to imply, says Gleason,☆ that there is a *proportional* relation of divergence to time, that in this form the principle is basic to all of lexicostatistics, and that it applies to all of language. He cogently rejects each of these implications.

4. In terms of these definitions, the *Dictionary of Linguistics* (Pei 1954) identifies first one aspect of lexicostatistics (determining genetic relationship by statistical comparisons), then glottochronology proper. Marshall (1956) restricts "glottochronology" to the lexicostatistic type, and extends "lexicostatistics" to the sense I have given "glottochronology," the study of rate of change in all of language. Gleason (1959:23) likewise restricts glottochronology, but does not extend "lexicostatistics" so broadly: "*lexico-statistical methods* are all those which depend on the comparison of two or more vocabulary samples selected on the basis of some lexico-statistical gloss-list, on the counting of certain types of similarities and differences between such samples, and the calculation from these results of certain measures of relationship." Voegelin's distinction has similar import (1958:58).

5. Note that 10.5% of the 200-item list do not appear at all in the written materials for the first grade sampled by Rinsland, and 1% (*guts, louse*) do not appear among even the first six thousand most common English words from adult written materials (Eaton 1940).

6. As a more important reason for rejection, Swadesh☆ suggests that "*three* may have appeared relatively late as an independent word in many linguistic groups (perhaps about 5,000 years ago) and not yet in some languages."

7. But we do not have a universe of several thousand items to sample, despite some early optimism (Lees 1953: 114).

8. Gleason☆ observes: "Since all the test cases were reasonably close to one millenium, they would be expected to get a reasonable approximation to the correct value for one millenium. (That is, the errors introduced by the fact that they are not exactly one-millenium spans in every case would be small compared to other sources of statistical variability.) The real trouble, and it is profound, is in the means suggested to go from a rate for one millenium to a rate for widely disparate periods."

9. Milke☆ comments: "The stress laid on the need for basic research as against premature and cheap 'applications' is really well done."

10. Hjelmslev attacks glottochronology as contrary to the comparative method (1958:633). He protests against the same common meaning as a criterion for judging glottochronologic sames (e.g., *deer:Tier* are not sames), on the grounds that semantic facts have nothing to do with the methods of determining genetic relationship, the concordances of which are established by expression (phonology) alone. Either this is meaningless, or it is false. Sound laws are based on cognates, and judgments of cognation are based on correspondence of both form and meaning. If correspondence in either regard is unsatisfactory, a likely cognate or etymology will be rejected; the literature has many instances. A concise, brilliant exposition of the sort of reasoning, both phonological and semantic, is given by Thieme (1958: 513–15). Hjelmslev also finds it strange, nearly unbelievable, that glottochronology excludes affixes, structure-bound elements, from the test list, and, again, because of the procedures of determining genetic relationship. He points out that, from its beginnings, genetic relationship has insisted on morphological elements more than on roots, and he insists, with Meillet, that "genetic relationship more easily, and methodologically more effectively, gets its bearings on the basis of affixes, which most of the time furnish more solid material" (Hjelmslev 1958:663). I think there can be no question of the correctness of his reading of the history of genetic relationship (cf. Twaddell [1948], Meillet [1958], and references cited in Hymes [1956*b*,*c*]), but it has nothing to do with the criteria for a test list. Rightly or wrongly, certain items are excluded because their usage has been found "too closely bound up with peculiarities of language morphology" (Swadesh 1955*b*: 126), so that single, clear translation equivalents often could not easily be gotten. In sum, the results of the comparative method are a prerequisite for valid glottochronology, but the criteria pertinent to each are not identical; Hjelmslev has misunderstood the relation between the two.

11. My discussion of these possibilities has been stimulated by Floyd Lounsbury.

12. Voegelin (1958:53) notes the existence of inconsistent results, but gives mistaken examples. The supposed Uto-Aztecan discrepancy between Hale and Swadesh is based on comparison between Hale's mistake in citing 4000 instead of 4700 minimum centuries, and an early Swadesh date using a different list. As noted, Swadesh and Hale obtain the same maximum divergence time when using the same (100-item) list. As to Athapaskan, I am unaware of any controversy between Hoijer (1956*b*)

and myself (1957*b*); my recalculations were an addition to his fundamental study, if in one respect a revision, and were accepted by him before the paper was submitted for publication. I had thought that the text and footnote of the paper precluded any impression of controversy with the teacher to whom I owe my knowledge of Athapaskan.

13. The same result was obtained independently by Swadesh (1958:763) and is employed by Lamb (1958:99).

14. Li (1959) rejects phonological isoglosses in favor of vocabulary.

15. Longacre (1957*a*) also points out that the implied sub-grouping does not fit that worked out by Hall (1950); the divergence times link French more closely to Italian than to Spanish, whereas Hall's family tree has the reverse.

16. Arndt is notably circumspect and cautious in applying the method.

17. I in fact consider Sapir's Penutian valid, on the basis of the sum of evidence in Swadesh (1956*a*, 1956*d*, MS.). Pitkin and Shipley (1958), Sapir and Swadesh (1953), and Hymes (1956*a*, 1957*c*, MS.)

18. Hattori☆ reports his use of the 100-item list for an interesting comparison of Japanese with eighteen other languages of Asia and the Pacific. The results are not yet analyzed into resemblances due to genetic relationship, diffusion, and chance. On their face, they seem to support the hypothesis of genetic connection of Japanese with Altaic (Korean, Manchu, Mongol, Tatar) and Ainu. One would like to know if the Chinese resemblances are loans, and if so, at what average per millenium.

19. Hall shows that the retention rate assumed for glottochronology "is not valid in case pidginization has intervened" (1959:266–67). He continues (p. 267), "and, since no one can tell whether this has been the case or not with languages whose previous history we do not know, the entire application of glottochronology to language families without documented history becomes doubtful." I agree that the usual retention rate does not apply to the products of pidginization, since, with Douglas Taylor, I do not consider these to be connected historically with their dominant source of vocabulary in the same way as ordinary languages are connected with their prior stages (e.g., English with proto-Germanic). A comparative study of available cases of pidginization might show a different regular rate of "retention," one much lower than that found for other cases. I disagree with Hall's conclusion that pidginization places the application of glottochronology in doubt, since I regard pidginization as a process specific to certain socio-economic circumstances, whose occurrence in such areas as the New World before the period of European expansion is practically out of the question.

Comments

By Harold E. Driver☆

[1] I should think that more use could be made of reconstructed proto-languages in choosing glosses for the list of basic words. The meanings shared by a dozen or more proto-languages would be historically stable, and the terms for them would be geographically widespread and likely to be found in almost every language.

[2] It seems to me that more energy, time, and funds should be channeled into validating the method from documentary evidence in Indo-European, Semitic, Hamitic, Sinitic, Uralic, and other language families where sufficient dates are available, and that less attention should be given to American Indian and other preliterate language families at the present time.

[3] At the same time we should not delude ourselves into believing that a more careful validation of the technique on the control languages will assure us of infallible accuracy in inferring time depths for preliterate languages. Any sample drawn exclusively from written Old World languages can never be proved to be representative of all the languages in the world. In the language of the statistician, the control languages will always be a selected sample of all languages, rather than a random sample, and their representativeness will always be subject to doubt although not necessarily faulty. All formulas of probability statistics are dependent on a representative sample of cases from the universe for which the generalizations derived from them are intended to apply. There is no magic by which results validated on a selected part of a universe can be extended to the whole universe.

[4] However, for those who are willing to make such an extension from a selected sample, it should be pointed out that it is not necessary that all the control languages change at the same rate. It is only necessary to know the average rate of change, and the standard deviation about the average, in order to estimate a time depth. If in the future the standard deviation turns out to be two or three times as great as Lees (1953) found it for his 13 languages, the time estimate can still be made from the average rate of change, but the standard error after the plus-and-minus sign will be two or three times as great. This would require building into the formula a standard error for a variable rate of change. If the empirically determined rates of change from, say, fifty or more control languages show a bi-modal or multi-modal inclination, this would tend to increase the error of time-depth estimates.

[5] We should, ideally, have more tests of the reliability of eliciting from informants and of matching cognates, by comparing the results of two or more linguists working independently on the same languages. Such replication is standard procedure in laboratory sciences.

[6] As Hymes indicates, the quantitative relationships between languages, as displayed in square tables, have utility other than as indicators of absolute time depths. They can offer provisional classifications of languages and relative time depths. Such numerical expressions of linguistic "distance" may also be compared with measures of cultural "distance" and biological "distance," and may eventually yield valid correlations between race, language, and culture. Boas' caution that these three attributes of man do not generally correlate to a high degree has discouraged people from looking for the modest but easily significant correlations that obviously exist and have relevance to historical problems.

It is becoming increasingly apparent even to non-linguists that the family-tree concept of linguistic change is too simple to account for many linguistic relationships. Although the square table (correlation matrix) exhibiting the relationship of every combination of two languages in a group is only two-dimensional on paper, it is important to remember that variation in the coefficients it displays can be multi-dimensional. In other words, no matter how complicated the relationships among a group of languages are, this form of expression can take care of the complexities. Various other kinds of diagramming can only approximate what the square matrix of coefficients shows accurately. Correlation matrices, therefore, have many functions in science other than as indicators of absolute chronology, and their use should be encouraged. The results of glottochronological studies can be very useful to the anthropologist without his worrying about the validity of the time depths in years.

By Isidore Dyen☆

I would like to comment on the subgrouping and dialectological aspects of lexicostatistics and to speculate on their relation to the glottochronological as-

pect as well as on frequency as a possible basic factor in the success of lexicostatistics in the latter aspect.

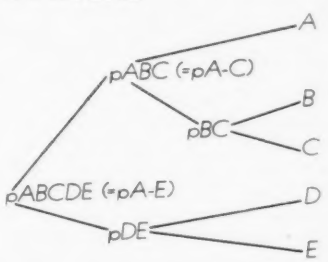
[1] Until the present, lexicostatistics has been used for three different purposes: (1) to establish the relationship between languages, (2) to classify (subgroup) related languages, and (3) to establish the times at which related languages began to diverge. It is only in its third aim that it has a unique contribution. It is also in its third aim that it is purely statistical. In its other two aims, it is a convenient device for making a statement about the relations shown by the basic-vocabulary lists of different idiolects. As such, it is a new instrument in the classifier's hand. It is a quantitative procedure, whereas hitherto the classifier generally worked only with qualitative arguments.

The difficulty with qualitative arguments in sub-grouping appears in the phenomena explained by J. Schmidt's wave hypothesis. He "showed that special resemblances can be found for any two branches of Indo-European, and that these special resemblances are most numerous in the case of branches which lie geographically nearest each other" (Bloomfield 1933:317). If, however, the development of a language boundary between languages restricts the influence of one language upon another to traits that are not employed in genetic classification (e.g., phonetic similarity of phonemes, similarities of syntactic and morphological structure, lexical borrowings), the phenomena explained by the wave theory must apply at a time before the languages were distinct. In view of the distribution of the special resemblances, the conclusion was reached that the Indo-European languages differentiated for some time as sub-areas or dialect areas of the same language. Since similar conclusions can be obtained by the use of lexicostatistical comparisons much more quickly and systematically, it is worthwhile considering this technique and its implications seriously.

There appears to be little opposition to the notion that lexicostatistics can be employed as a technique in sub-grouping (or internal classification) of related languages. This procedure, like the traditional one, utilizes the assumption that if two languages are relatable, they exhibit some cognate morph sequences. It is therefore possible to construct rules for the admission of morph sequences as cognate.

We can regard the traditional approach to sub-grouping as basing itself on special resemblances (i.e., similar features distributed over less than all of the members of a family). Wherever these appeared in large numbers for some sub-set of related languages (e.g., Indian and Iranian), the conclusion could be reached that the probability was correspondingly great that some of the special resemblances were exclusively shared innovations (ESI). It was of course possible that some of the special resemblances (SR) might be residual (i.e., inheritances), but the probability that the mass of SR's included ESI's was regarded as increasing with the number of SR's. Furthermore, as the number of ESI's increased, the probability that there existed a private proto-language was regarded to be increased, since it was increasingly likely that the source was a distinct language. Consequently, it followed that if a number of languages shared some large number of SR's with each other, and for each of the sharing languages this number was significantly larger than that shared with any other language than one of the group, the existence of a private proto-language could be inferred.

Let us suppose that the true family tree is as follows:



Each of the words of B relevant to the family tree is cognate with at least one word in another language. Any word of B that is cognate with some word in the other language can be assigned as follows:

1. To pA-E if cognate with a word in D or E.
2. To pA-C if cognate with a word in A and not with a word in either D or E.
3. To pBC if cognate only with a word of C.

For the family tree to be correct, it might appear that the sum of the cognates of B shared with an immediately related language must be greater than that with a more distantly related language. There would then be a logical argument for regarding a lexicostatistical classification of related languages as representing the order of separation of any collection of distinct languages.

However, this need not hold true in every case. It is conceivable that of two languages which are (stipulated to be) immediately related, one would specialize in the loss of inherited words, and the other in the loss of innovations. The resultant percentages would be misleading. It is thus not possible to regard the lexicostatistical percentages as a purely qualitative criterion. In using a lexicostatistical list there is a dependence on the probability of randomness in the replacements, so that it is to be expected that both innovations, and in general inherited words, are not distinguished in the probability of their being replaced.

It is possible that the words assigned to pBC are in every case direct inheritances from pA-E. We assign them to pBC because this assignment is positively indicated. Wherever a reconstruction is made, it is necessarily a minimal reconstruction. If a new related language should be discovered, it would not be impossible that we should have to assign some of the words now assigned to pBC to the earliest proto-language.

Consequently, the immediate relationship of B and C depends on the probability that at least one of the words assigned to pBC is an ESI. This probability is regarded as increasing with the number of items assigned to pBC. If the items are about equal in value, the number of items assigned to pBC is an index of the probability that at least one of them is an ESI.

If two non-defective Swadesh lists of different languages are compared, there are the following classes into which the paired entries fall. They are a *comparable* pair (C) if both entries are known. They are a *determinate* pair (D) if their cognation can be decided. They are a *positive* pair (P) if they are cognate. The ratio or percentage which we regard as interesting is P/D. This ratio is called the *cognate percentage* or *percentage of cognates*. In terms of our family tree above, it is for B and C the sum of the words assigned to pA-E, to pABC, and to pBC over all those pairs of words whose cognation could be determined.

We can now develop a series of propositions which parallels the development presented above as the traditional one. Since the number of items assigned to pBC must increase in proportion to the number of ESI's, the number of items assigned to pBC is an index of the magnitude of the quantity of ESI's involved. The cognate percentage of the lists B and C is in direct proportion to the number of words assigned to pBC. It follows that the difference between the cognate percentage of B with any other language is an index of the probability that B has at least one ESI with C and similarly for C. The smaller of the two differences, or, if they are

equal, the amount of the differences themselves, is an index of the probability that B and C have no ESI's with any of their partner's competitors, and thus that their mutual proto-language was distinct from any continued by a language represented by a list.

An essential difference between the traditional approach to sub-grouping and the one employed in lexicostatistics is that the traditional approach aims to identify the SR's, and infers the immediate relationship—as dialect neighbors or as constituting a sub-group with a private proto-language—from the relation between the amount of the SR's shared with each other to the amount each shares with other languages. The lexicostatistical procedure infers the existence of ESI's for any sub-set of related languages from the fact that the members of the sub-set have their highest percentage of cognates with each other. The existence of a private proto-language is indicated by (1) the fact that for any member the percentages held with related languages outside the sub-group are about the same for each member of the group, or by (2) the chain of highest percentages is interrupted. In the latter case, the conclusion of a private proto-language must be regarded as provisional, because a hitherto unrepresented language could be discovered to be a missing link.

Thus an important difference between the traditional approach to sub-grouping and the one employed in lexicostatistics is that the traditional approach attempts to identify the special resemblances—if at all possible, and inevitably a long drawn-out procedure—whereas the lexicostatistical approach is to trap special resemblances in the lexicon without necessarily identifying them. The identification then becomes an implication of the sub-grouping in lexicostatistics, whereas the sub-grouping is regarded as an implication of the innovation in the traditional procedure.

It is likewise an assumption of the lexicostatistical procedure that its results imply the morphological developments, i.e., that the developments in the morphology are expected not to contradict the results of a lexicostatistical sub-grouping. If an apparent contradiction is discovered, there is an error in interpretation. For example, what has been taken to be an innovation may actually have been an inheritance, which perhaps occurred over a restricted area or the like. If obvious morphological innovations do contradict a sub-grouping, they can be regarded as independent innovations. However, as the number of independent morphological innovations implied by a lexicostatistical sub-grouping increases, the more improbable are the implications of that sub-grouping. We can therefore regard morphological innovations, and likewise syntactic innovations where these involve cognate morphemes, as constituting an independent negative estimate of the reliability of the implications of a lexicostatistical sub-grouping.

Whatever difficulties we face then in determining the order of separation arise only when we are not dealing solely with distinct languages. Languages that are not distinct are most simply regarded as the same dialectological language.

A problem might arise in dealing with a language that covers a large geographical area and is highly dialectalized. It is conceivable that in its spread different languages could have separated from it at different points in its territory. If the extensive language is A, and the offshoots are B and C, it is possible that B is similar to some dialects of A, and C to others, and the family tree that results might depend on the dialect of A that was chosen as representative. A correct solution would require that the relation of any two languages be determined by the percentages scored by their most closely related dialects.

It is conceivable that two different languages would retain all the basic vocabulary of their proto-language unchanged. In that case, no list of basic vocabulary would apply to determining the degree of their relationship. It is an assumption of lexicostatistics that if two lexicostatistical lists score 100%, they are taken from the same language. If they are nevertheless different lists, they are from different idiolects of the same language.

In fact, some linguists are ready to tolerate a much lower percentage than 100% before regarding two lists as perhaps not belonging to the same language. The cut-off point needs to be established. This cut-off point can be defined as the lexicostatistical language limit, for there would be no two languages with dialects that shared a percentage of cognates higher than this.

There could, however, be languages with dialects that scored lower than the language limit. These dialects would belong to the same language by virtue of the fact that there was a chain of dialects from one to the other, such that one could reach any member of the chain by passing from one dialect to another with which it scored more than the language limit.

It is even conceivable that in a limited examination a pair of dialects in different languages would show their highest percentage of cognates with each other, without their being immediately related. Suppose that two languages were from archaizing dialects in their respective areas, and the sign that they were archaizing was the retention of basic vocabulary. The consequence might be that if we were to examine only the lists without regard for language boundaries, we could end up by assigning two dialects as being immediately related when actually their immediate relationship would be quite different.

Now this circumstance could occur if at least one of the dialects belonged to an extensive language area in which the only other representatives from the same language area were so distant as to be different in a degree greater than a dialect belonging to a related but different language.

It is easy to see that this error would have been avoided if a dialect intervening between the two extremes had been included among those investigated.

Even when distinct languages are involved, rather than dialects, the same problem could conceivably arise. For it is possible for a language area to break up into different languages at approximately the same time in different sections. If this happens, it would be reasonable to expect the language that continues the intervening dialect area to show immediate relationship equally with two languages, whereas the extreme languages might show a relationship that was as remote as, or more remote than, they might show with still other languages.

[2] It follows that any sub-grouping is dependent on the particular collection of lists employed. It is therefore possible to regard the entire world of speakers as a collection in which each speaker is represented by a list. The classification of all speakers by relationship can be achieved simply by comparing the lists for cognates. If we can recognize a language boundary wherever one occurs, the boundary of languages with the smallest number of cognates can be termed the minimal cognate boundary. If lists differ by fewer cognates than the minimal cognate boundary, they belong to different languages. The minimal cognate boundary is a lower bound of the lexicostatistical language limit defined above.

In view of the fact that in most discussions of lexicostatistics, the distinctness of the sources from which the lists are drawn is left out of account, it is wise to specify that the result is a classification that will hold true if the sources are distinct. We will call this a QD (i.e., quasi-distinct) classification.

Now a QD classification is different from a classification by special resemblances, in that there is a quantification. In this quantification, the elements are treated as equal. In classifying by cognates, we make absolute determinations

of the presence or absence of a cognate set member. In the traditional application by comparatists, it is not clear what the position of the meaning is in such cognate sets. In the lexicostatistical procedure, the requirement is that the meanings be the same before cognates receive consideration. Under these circumstances, cognates with similar but not the same meaning are disregarded. Furthermore, in a QD classification it is of no concern which words are replaced. The precise number of replacements furthermore plays no role, so that in our family tree it would make no difference whether AB = 40%, AC = 30%, and BC = 60%, or AB = 70%, AC = 40%, and BC = 90%. The same relation would be indicated by the family tree.

This disregard permits the use of defective lists. A defective list is one that lacks an entry to match that of another list. It is easy to see, however, that in the worst case the employment of defective lists can lead to worthless results.

The reliability of a comparison of lists can in fact be measured by the number of comparable entries in relation to the number of items in the standard list, the decrease in reliability gradually increasing with each unused item. Thus 100 pairs of homosemantic entries, for lists with 200 entries as standard, is less than one-half as reliable as 200 pairs.

Defective lists can be employed with least risk when each of the items is equally subject to replacement. For example, if defective lists are collected in such a way as to favor words that are less subject to replacement than others, the resultant percentages must differ noticeably from the percentages resulting from the use of more nearly complete lists. If 100 words are taken in the collection of a 200-word list, it is to be expected that the more common words will make up a large part of the 100. If the more common words resist replacement to a greater extent than less common words, the outcome will be skewed.

In fact, it is reasonable to suppose that the more common a word is, the less likely it is to be replaced. The replacement of a word by another is a change in their respective frequencies. Let us imagine what happens when a word A replaces another, B, in a given meaning M. If the meaning M is regarded as being constant in frequency, then what must happen is that this frequency is shared by the two words, at first in a proportion that favors B, and later in one that favors A. If other factors remain constant, then it would appear reasonable to regard the difference in frequency of B over its nearest competitor A in the meaning M as the index of B's resistance to replacement in that meaning.

Now the meaning M itself could change in frequency. As M loses frequency, B in that meaning must necessarily lose frequency and thus perhaps become less resistant to replacement in that meaning.

For a given meaning M at a particular time, in a particular language, there is possibly only one word. In general, however, since we cannot even limit the competitors to single words as against phrases, it is to be expected that there is at least one competitor for each meaning. As the factors that favor the increase of frequency of one word over another change, it is possible that the frequency of the competitor will increase.

Now some meanings are not found in some languages. It is not that there is no word for that meaning, but simply that the meaning does not exist. It is not hard to believe that among the speakers of some language, there has never been an occasion to refer to lexicostatistics. This is not to say that they would not have a word for this should the occasion arise. But until the occasion arose, it would be fair to say that the meaning did not exist in their language.

In choosing the items for the lexicostatistical list, the effort is made to pick out such as could be found in all languages. This has the practical effect of ensuring comparability. Now it is important to recognize that we are dealing with meanings rather than words. As the application of lexicostatistical lists is widened, it may prove necessary to modify the particular meaning-item. This could occur if a given language offers two or more non-synonymous words in a given standard meaning. There is no word for "animal," but only one for "wild animal" and one for "domesticated animal." There is no word for "cut," but a large number, each of which specifies some feature involved in the act. In effect, to choose one of these, is to make the choice for all lists. The meaning-item is thus restricted.

It is useful to regard the universality of a meaning as a prime prerequisite for membership in a lexicostatistical list. Yet this is not the only prime prerequisite.

Frequency of the entry plays a role in its choice. This appears in the simple suggestion: *When there are two or more words for a particular meaning, choose the more common one,* a phrase which can be translated into "the more frequent one" without harm. But universality is itself the criterion that there be a frequency of the meaning greater than zero in each language. Thus frequency is a prime prerequisite.

But frequency appears also in a different way. We can regard as the population from which a lexicostatistical list is drawn all the most frequent words for meanings that have some frequency in all languages. For the purpose of approaching as closely as possible the proposition that in two particular instances a figure of 40% should have the same significance, it would be highly desirable to have the items for which entries are sought to be meanings that generally show the same resistance to replacement. We should therefore regard as a criterion of our items a relatively similar average frequency in all languages.

It is possible to entertain as a working hypothesis that the frequency of universal meanings is culture-free. The implication is that the frequency of all English words in the meaning "organ of sight" is the same as the frequency of all Malay words in that same meaning. If the English word 'eye' in this meaning has about the same frequency as the Malay word *mata* in this meaning, this could be regarded as evidence, provided of course that their respective competitors did not show excessive frequencies.

The contrary of this proposition is that the frequency of these meanings is not culture-free. We know that the frequency of some meanings is not language-free. For example, a verb in Malay need not be accompanied by a subject. The frequency of the word for the speaker ("I") is no doubt thus affected. In the absence of the word, it would be difficult to argue that the meaning "I" had occurred. It is simpler to conclude that the meaning had not occurred (i.e., the subject was not "omitted"), but that the hearer makes an inference (which could be wrong) from the preceding (and sometimes following) utterances. Under this interpretation, the frequency of the meaning "first person" might be lower in Malay than in English. In at least this sense the frequency of meanings is not culture-free. Furthermore, it is not difficult to imagine a particular culture in which a special attribute of, say, the organ of hearing ("ear") would make this meaning more frequent than in some other culture.

If we were to take an average frequency of a particular universal meaning, an excessive variation above or below this average would have to be culture-bound. It is reasonable to regard the meaning "airplane" as a universal. If this meaning does not exist in all the languages of the world at the present moment, it is hard to see how this will

not soon become true, unless this mode of transportation should soon be made obsolete. However, its frequency might vary tremendously from that in the speech of an isolated group which rarely sees an airplane, to that of a people which sees one frequently. The low frequency and the high frequency are culture-bound.

An average frequency is culture-bound whenever there are super-average and/or sub-average frequencies spread over a large number of distinct languages. If an average frequency is also associated with a large number of distinct and unrelated languages, then it can reasonably be called a culture-free average. Basic vocabulary for a given language can perhaps then be defined as the collection of the most frequent words for each of the meanings with a culture-free average frequency.

Different meanings show a difference in frequency. In correlation with this, words and phrases show a difference in frequency. In fact, a language can be conceived of as a list of its meaningful constituents ordered by frequency. It would indeed be highly surprising if the relation between the relative position of partly homosemantic constituents in their respective lists was random. If basic vocabulary is determined by meanings with a culture-free average frequency, this relation cannot be random.

If we regarded the English words that satisfy the criterion set up above as having, by and large, frequencies close to the average, it would be possible to approximate a homogeneous set of words by selecting according to the English frequencies and then testing for similar (or at least correlated relative) frequencies of their counterparts in other languages.

Since we intend to apply our lexicostatistical lists to earlier periods, we will attempt to make the members not only contemporaneously culture-free, but also as nearly culture-free throughout time as possible.

[3] Dialects as well as (distinct) languages can be QD classified. The usefulness of this classification depends entirely on the purpose to which it is put.

It has long been a conviction of dialect geographers that the best way of attacking the classification of dialects is via the vocabulary. At present the only competing type of classification is that based on phonetic (or phonemic) changes. There is no compulsory relation between the two types of classification. If two idiolects are classed together in one classification, it need not follow that they will be classed together in the other.

If dialects differ by a phonetic change, there is interference in communication between their respective speakers. If dialects differ by an item of basic vocabulary, there is likewise interference in communication between their respective speakers. There is no known way of regarding different phonetic changes as commensurate with each other so as to be able to measure the interference with communication which they cause. Items of basic vocabulary, to the extent that they are otherwise homogeneous, are commensurate, and thus suggest an ideal index of communicational interference between dialects. This interference can be regarded as the entropy of the system.

It therefore appears reasonable to index the interference in communication between dialects in terms of the frequency in the same meaning of non-cognate words. We consider only non-cognate words. If word D_{1i} in dialect D_1 is cognate with, and has the same meaning as, word D_{2i} in dialect D_2, then the meaning of D_{2i} can be inferred from D_{1i} by examining the results reached by applying the rules of phonemic transformation (= the rules of phonemic correspondence regarded as an operation). It is likewise reasonable to believe that the effects of analogy are normally not sufficient to impede this inference, and can therefore be disregarded. Thus cognateness is regarded as unaffected by analogic change.

If the average frequency of homosemantic non-cognate basic vocabulary is an index of the interference in communication between speakers of different dialects, then we might wish to seek whether in fact the progress of this interference is in any way correlated with time. The approach thus far has been empirical, and has met with just enough success to warrant further research, but not enough to put the matter beyond criticism.

If the resistance to replacement of a particular word in a particular meaning is the difference in frequency of that word in that meaning from the frequency of its nearest competitor, then to get a simple correlation with time we should wish to use meanings for which the ordinary words have in most cases the same resistance to replacement. The need for communication places a general limitation on the number of replacements that can occur relatively close in time in the same system. It is clear that all of our experience suggests that we should look at the system as being one in which each word is under a pressure for replacement and a pressure against it, but that the pressure for replacement (entropy) is sufficiently great so that it wins through at certain points.

We have reason to believe that the points of break-through are not entirely random. If words of high resistance are actually less likely to be replaced, then if the list chosen contains words of varied resistance, the break-through should normally occur more often in the words of less resistance. Over long periods, it would be increasingly likely that the same word would be replaced twice. If this factor is not taken into account in the calibration, an inevitable distortion will occur in the application of a relatively short-term calibration to a relatively long one.

Some who use lexicostatistics use the calibration established by Swadesh and Lees, but use different words. If there are differences in susceptibility to replacement among different words, the risk of error involved in this procedure is increased.

The possibility of multiple replacements increases the variety of the effects of a constant in different language continua and over different spans in the same continuum. For this reason, the study of replacements over shorter periods than a thousand years may provide results important for the establishment of the constant.

Among the words in the Swadesh diagnostic list, there are a large number with high resistance to replacement. This is demonstrable not on the basis of their relatively high frequency, but on the basis of their known persistence in control languages. Nevertheless, there are also included in the diagnostic list some large number of words which by the same token have a much lower probability of persistence.

It is interesting to note that in the Swadesh 100-word list (1955*b*), the range of variation in persistence values has been reduced (from approximately 1%–100% for the 200-word list) to 21%–100%, and that the concentration of persistence values is noticeably high at the upper end. This can be regarded as an improvement. Since we are, however, interested in variation, rather than in persistence, a more useful list would show fewer or no extremely stable items. Since we are furthermore interested in as nearly random as possible a distribution of the replacements, it would also show the characteristic of having a relatively large number of meanings concentrated over a relatively small percentage, say two or three decades.

Various lists could be used for special purposes. For example, one could be designed for the dialectological analysis of a language. Here one would prefer a very sensitive list, in which the words were relatively equal in relatively low resistance to replacement.

For sub-grouping, it might be desirable to have a number of lists of different degrees of resistance according to the precision desired. For the determina-

tion of the order of the most distant relationships particularly, the calibration of lists of the most resistant words would be highly desirable, whereas for those of closer relationships a more sensitive list would be more useful.

[4] The choice of range of persistence values would be different for the demonstration of relationship. The aim in a proof of relationship is to demonstrate the probable existence of a pair of cognate morphs in the languages being compared. Since a pair of cognate words is also a pair of cognate morph sequences, it is useful to examine the words of the language which probably have the highest resistance to replacement. For cognates should appear among these if anywhere among the words of the language. Among those languages the relationship of which is regarded as proved, it is unlikely that there is a pair of languages without a single pair of homosemantic cognates.

Swadesh has made it clear that it was one of his hopes to convert his lexicostatistical test, by itself, into an argument for relationship. However, to do so he must inevitably use a test other than systematic matching of phonemes as a criterion of a cognate-like relation. This he supplies by a rule of similarity (1954*a*: 319ff.) There is, however, no way of demonstrating that his particular rule of similarity is sufficient.

In fact, to use a lexicostatistical list in this way involves the investigation in the greatest risks, and is unnecessary when the languages themselves are accessible. The value of a comparison is greatest as the interval for which it is reliable grows. Because of linguistic change, however, the probability of recognizing cognates must decrease as the interval grows, even when entire languages are subjected to comparison. If the material examined by the comparatist is restricted to the entries of a diagnostic list, the chances of making a correct determination are greatly reduced. The list should therefore not be used alone in a proof of relationship. Since, however, the list does contain basic vocabulary, its contents can be used like any other part of the language which is more likely to exhibit a retention.

The use of a lexicostatistical list in the proof of relationship is thus different from its use in sub-grouping and in glottochronology. For proof of relationship we should be interested in the morph sequences of highest resistance. Since the proof of relationship depends on no more than the proof of the existence of a single pair of cognate morph sequences, the examination of the words of highest resistance is among the first steps to be taken by the comparatist.

By SARAH C. GUDSCHINSKY☆

[1] At the time I wrote 1956*b*, I saw only one generalized use for the techniques of glottochronology: to provide a time-depth estimate, with implications of sub-grouping as an incidental by-product. I would now like to register a change of viewpoint. As I now see it, there are at least three different uses of glottochronology, with at least some difference in technique required by each. Glottochronology can be used on raw data for the purpose of postulating possible relationships and degrees of relationship; it can be used following careful comparative study either for the purpose of estimating divergent time, or for establishing a sub-grouping of related languages. Each of these uses is discussed at some length by Hymes, but there are a few points that I would like to especially emphasize.

According to my present view, the first usefulness of a series of word lists is for the setting up of a skeleton classification to guide later research, i.e., the lists are compared and instances of greater than chance resemblance are assumed to imply genetic relationship. These resemblances are quantified (simple percentage at this point is fully as useful as divergence time, and undoubtedly less misleading); these quantified results permit a tentative grouping of the languages into sub-groupings with varying degrees of supposed relationship.

Such groupings in turn provide the outline for further exploration of the relationships, and for the detailed and painstaking work that is required for establishing relationships. The wide-ranging use of glottochronology facilitates field work by narrowing the range of search for relationships at a given level.

A point of particular emphasis is that sub-grouping and estimation of time depth are legitimate only *after* careful comparative work has proved the relationships, and only on the basis of proved cognates (i.e., cognates that are elements in reconstructions). This means that the use of "probable cognates" or guess at "glottochronological sames" should be limited to studies that are setting up hypotheses, and not used in the more exacting task of measuring time. This is not intended to imply that sloppy work is adequate for setting up the initial hypotheses. It would probably be generally agreed that the validity of such hypotheses is in direct proportion to the skill, experience, and—perhaps—the intuition of the investigator who proposes them.

At this point, however, I specifically reject the notion that the discovery of more resemblances than would be expected on the basis of chance constitutes in itself "proof" of genetic relationship; and I seriously question the more basic notion that all evidence of relationship is primarily statistical. There would be a great deal of disagreement, of course, as to how much of what kind of evidence constitutes complete proof. But I would consider a relatively small body of patterned similarity (i.e., material for reconstructing a plausible phonemic pattern and part of the grammatical structure) more weighty than a much larger number of unpatterned similarities.

Lamb's terminology seems useful for discussing this difference between proved relationship and postulated relationship (Sydney M. Lamb, "Some Proposals for Linguistic Taxonomy," *Anthropological Linguistics* [1959] 1:33–49). Relationships postulated on the basis of similarities, but not yet checked by comparative studies (or not checkable by comparative studies) would be Probable Relationships; those for which the comparative evidence seemed conclusive would be Established Relationships.

[2] It would seem to me that there is somewhere a happy medium between the repudiation of the "mesh principle" and its over-emphasis. Mesh relationships are a result, not an explanation. Presumably every shared linguistic trait that is not of chance duplicate origin is the result of either homogeneity at the time it originated, or of later diffusion from its place of origin. It is well known that it is possible to recover some of the history of a language by careful comparison of the linguistic evidence with what is known of the historical, geographic, and cultural factors. To dismiss all this under the cover term "mesh relationship" is to obscure potentially useful facts.

An example may be seen in my Mazatec studies (1955, 1958). The Mazatec dialects present a typical mesh, with the dialect of San Miguel in particular exhibiting some characteristics in common with the Jalapa dialect, and some in common with the Huautla dialect. This statement, however, says nothing about the history of the San Miguel dialect, nor how the mesh originated. A careful study of the linguistic data permits us to say that the San Miguel dialect was originally homogeneous with the Jalapa dialect, but that it later shifted to a close attachment to the Huautla dialect. A comparison of these facts with what is known of the local

history makes it possible to correlate these linguistic shifts with the political changes, and to "explain" the occurrence of this particular mesh.

[3] I suspect that the problems involved in filling out properly the diagnostic list may be sometimes greater than is usually realized. I would suggest, for example, that when Rae uses *garra* as the Spanish equivalent of *claw*, and Swadesh (supported by Taylor) uses *uña*, the problem begins with a basic difference in the referent for the English word *claw*. *If* these men speak the same dialect of Spanish that I do, Rae is thinking of *claw* as referring to the entire clawed foot, while Swadesh and Taylor think of *claw* as referring only to the animal analogue of a fingernail. This difference appears to occur in some dialects of English, as I have noted it among my colleagues in discussion of this item on the test list.

I have recently found in checking with colleagues that there are other words on the 200-word list for which we consistently use different areas of meaning. This means that in addition to the difficulties already reported in finding simple meanings in various languages, there is also the difficulty of being sure that each investigator is working with the same meaning.

In this connection, I might add that I think Taylor was too harsh in his judgment of Rae. Within the *campesino* dialect of Spanish which I learned in rural Mexico as a by-product of my study of Mazatec, Rae's "translation" of *pájaro* for *ave* is not only the most common, but probably the only, choice: his *quemar* for *arder, camino* for *vareda, pecho* for *seno,* and *pescozo* for *cuello* seem to me far more common. On the other hand, his *pequeño* for *chico* seems to me less common.

But there is something much deeper involved here than the simple question of whose variety of Spanish is more acceptable or more common. Certainly for all twelve of the words in which Rae's choice disagrees with Swadesh's, both choices are acceptable Spanish, and would be understood by all speakers at least of Mexican Spanish. But suppose that two lists were filled out in different parts of Mexico. And suppose that these hypothetical lists differed in every case where there is such a choice (i.e., a minimum of twelve differences). And suppose that each investigator involved were convinced that he had the commonest equivalent for the area or dialect that he was working with. Note that the difference between these two "dialects" would be nearly 600 years! And such a difference could be found in an almost completely homogeneous linguistic community! It should also be noted that the wide knowledge of Spanish that might safeguard us in considering Romance data (that caused Taylor to question the propriety of Rae's translations) is lacking in many of the American Indian languages in which we are currently using the lists.

All this leads to the question, "What does glottochronology measure when it gives diverge time?" Does it measure the point at which dialects began to diverge? Or the end of effectual communication? (No, this is not possible, since we can get divergence time for dialects that are still mutually intelligible.) Eventually, it seems to me, there needs to be some clarification of exactly what the time depths mean.

Along this same line, there is the question of what is meant by saying that in a group of divergence times for a single group of related languages, the oldest time is taken to be the more accurate. Is this intended to imply that successive splits are not possible?

[4] "It does not seem fair to conclude from a study so largely within this range that 'any hope of determining absolute time—by some improvement in the precision of lexico-statistical technique—seems to have been permanently lost.'"

This statement of mine should be read within the following context, ". . . since the order of differentiation can be deduced from the comparative material but the precise number of cognates lost and reborrowed cannot be so deduced."

The point is specifically that these may be changes in the basic vocabulary of such a nature that borrowed items are counted as glottochronological "sames" even by the most careful worker, and that in *such cases* there is no way of knowing that the items are borrowed, and hence no way of correcting the skewed time depth.

Hymes may be right, however, in assuming that such undiscoverable reborrowing is likely only in very closely related dialects such as the Mazatec under discussion.

By Shirō Hattori☆

[1] What I assume by proposing the replacement of 2 by 1.4 in the abovementioned formula, is that the factors of drift and dregs (these terms will be defined later) are always present even when that of contact is absent. Has the combination of these three not caused a too short glottochronological time depth for the Romance languages?

In my *The Affinity of Japanese* (1959), in a reprinting of a 1957 article of the same title, I discussed this point more in detail. I quote the discussion here, translating it from Japanese (pp. 199–202):

> I will show a clear example of the independent similar development of related languages without mutual influences after separation.
>
> Proto-Germanic *ā has been preserved as [a:] (and [e:]) in German, but has been changed into [u:] (and [i:]) in English. Proto-Germanic *ai has been changed into [ou] (and [i:]) in English, but into [e:] (and [i:]) in German. These are markedly divergent changes. On the other hand, both languages have dipthongized the Proto-Germanic long close vowels *i, *ū into [ai] [au], and changed the Proto-Germanic long half-close vowels *ē₂, *ō into close [i:] [u:]. Moreover, it is to be noticed that these dipthongizations cannot be due to a mutual influence, because they took place during the periods between Middle English and Modern English, and between Middle High German and Modern High German. This is a surprising coincidence.
>
> I thought that it would be of interest if we could establish that similar changes took place also in vocabulary, i.e., that both in English and in German the same items were independently replaced by cognate words. However, examples are apparently few, and I was able to find, out of Swadesh's 215-item list, only the following:
>
> The meaning 'to hold' was expressed in Gothic by *haban* [which corresponds to Latin *capere* 'to seize, take'], and in OHG by *habēn*, which became *haben* 'to have' in ModHG. On the other hand, ModHG *halten* 'to hold' comes from OHG *haltan* 'to keep, guard, observe, pasture.' OE *habban* meaning 'to have, hold,' and OE *healdan* meaning 'to hold, keep, guard, pasture,' have become ModE *have* and *hold* respectively.
>
> The meaning 'man (male person)' was expressed in Gothic generally by *wafr* [which corresponds to Latin *vir* 'man (male person')], and in OE generally by *wer*, but since that time, ME *man*, which meant originally 'person, human being,' took its place. In OHG the meaning 'man (male person)' was expressed by *man, goman, war*, but it is expressed by *Mann* in ModHG.
>
> The meaning 'right (vs. left)' was expressed in OE by *swiþra*, in OHG by *zeso*, but it is expressed by *right* in ModE, and by *recht* in ModHG, which came from OE *riht* 'straight, just, etc.' and OHG *reht* 'straight, just, etc.,' respectively.
>
> The meaning 'to wash' was expressed in OE by *þwēan* (parts of the body, dishes, etc.), *wæscan* (clothes, etc.), *swillan*. In ModE only *wash*, which came from OE, *wæscan*, has remained. OHG had *wascan* and *dwahan* (parts of the body, dishes, etc.), but only the former has remained as *waschen* in ModHG.
>
> However, it is very difficult to find such examples, and we have to think that "the tendency of independent similar development of related languages after separation" must mean in the field of vocabulary rather 'the tendency of independent retention (and loss) of the same words.'
>
> Anyhow, we have to examine empirically whether it is possible or not to use the formula $d = \log c/2 \log r$ [this will be done by using the formula $d = \log c/x \log r$, in reference to cases where we know d, c, and r], in order to contribute to the refinement of the method of glottochronology.
>
> It should be borne in mind that two dialects that are already different in phonology, etc., tend to slow down the spread of divergent development, especially in vocabulary, when they are adjacent to each other. The shift of consonants of HG began before the middle of the eighth century, and was completed at the end of the same century. Nevertheless, the glottochronological time

depth between Dutch and ModHG is very small. Using Swadesh's old list, I got 82.2% (residue) common to ModHG and Dutch. This means:

316 years in $d = \log c / 2 \log r$ (r:0.805)
451 years in $d = \log c / 1.4 \log r$

On the other hand, the great difference of consonant systems of HG and Dutch began to appear more than 1,200 years ago. The two languages should have influenced each other in the development of lexicon, although they were different in phonology. We can see similar phenomena between the Japanese dialects of Kyoto and Tokyo, and between the Chinese dialects of Peking and Suchow.

Therefore, even if English and German began to diverge *circa* 2,000 years ago, we should not put the figure 2,000 for d in the above formula. Because, before the Anglo-Saxons came to England in the latter half of the fifth century, they lived in the neighborhood of the ancestors of the Germans, and they probably influenced each other linguistically. (In 1954 I used provisionally the figure 1,800 for d [without any guarantee], and got the figure 1.4 in place of 2.) I should like to do similar research on Norwegian and Icelandic.

I should like to call "drift and dregs" the above-mentioned tendency of independent replacement by cognates, loss (= drift), and retention (= dregs) of the same words in a group of related languages. I think the exploration of drift and dregs in this sense, if there are any, is very necessary for the refinement of the method of glottochronology.

[2] I have been trying for several years to enlarge Swadesh's list, in order to heighten, if possible, the statistical reliability. Another objective of mine was to compile a list that may be more effective for exploring the proofs of genetic relationship between languages.

To start with, I compiled a pretty large body of basic vocabulary, including all the items found in the following lexicons. Several scholars collaborated in this work.

KOKUSAI BUNKA SHINKŌKAI, *Basic vocabulary of Japanese*, 1944.

DOI, KŌCHI, Nippongo no sugata, 1943, pp. 333–37.

AIIKU KENKYŪJO, Linguistic development of children, 1943, pp. 30–53.

SAKAMOTO, ICHIRŌ, *Basic vocabulary of Japanese*, 1943.

RAXMANOV, I. V. (Ed.), *Slovar'—Minimum po anglizyskomu, nemetcskomu i francuzskomu yazykam*, 1951.

MUEGGE, M. A., *German word-frequency vocabulary*, 1936.

BEACH, T., *French word-frequency vocabulary and sentence construction book*, 1933.

FREI, H., *Le livre des deux mille phrases*, 1953.

Ministère de l'Éducation Nationale, *Le français élémentaire*, 1954.

JOSSELSON, H. H., *The Russian word count and frequency analysis of grammatical categories of standard literary Russian*, 1953. (First half.)

COHEN, M., *Questionnaire linguistique*, 1950–51.

BUCK, C. D., *A dictionary of selected synonyms in the principal Indo-European languages*, 1949.

PALMER, H. E., *1000 basic words of English*, 1952.

Out of this body, I selected, in collaboration with Mr. H. Kitamura, the most basic items, excluding the following:

(1) Too specific items, e.g., "eyelashes," "pupil," "canthus," but "eye" is included.

(2) Items with abstract meaning: "meaning," "courtesy," "temperament," "to feel," etc.

(3) Items peculiar only to a few nations or civilized peoples. However, "snow," "ice," etc., are included, because they are common to many nations.

(4) Items that are very possibly duplicated in many languages, e.g., "food" is included, but "meal," "dinner," are not; "to work" is included, but "a work," "a job," are not; "near" is included, but "to approach" is not; "to steal" is included, but "thief" is not, etc.

Although we found some of them inappropriate, we have included all the items of Swadesh's old and new lists, in order to make it possible to compare the obtained material with that obtained by other glottochronologists.

Before I started the work, I intended to compile a list of about 1,000 items. But it turned out to be very difficult to find more than 500 items. Our list that was provisionally published in August, 1957, has only 457 items.

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not soon become true, unless this mode of transportation should soon be made obsolete. However, its frequency might vary tremendously from that in the speech of an isolated group which rarely sees an airplane, to that of a people which sees one frequently. The low frequency and the high frequency are culture-bound.

An average frequency is culture-bound whenever there are super-average and/or sub-average frequencies spread over a large number of distinct languages. If an average frequency is also associated with a large number of distinct and unrelated languages, then it can reasonably be called a culture-free average. Basic vocabulary for a given language can perhaps then be defined as the collection of the most frequent words for each of the meanings with a culture-free average frequency.

Different meanings show a difference in frequency. In correlation with this, words and phrases show a difference in frequency. In fact, a language can be conceived of as a list of its meaningful constituents ordered by frequency. It would indeed be highly surprising if the relation between the relative position of partly homosemantic constituents in their respective lists was random. If basic vocabulary is determined by meanings with a culture-free average frequency, this relation cannot be random.

If we regarded the English words that satisfy the criterion set up above as having, by and large, frequencies close to the average, it would be possible to approximate a homogeneous set of words by selecting according to the English frequencies and then testing for similar (or at least correlated relative) frequencies of their counterparts in other languages.

Since we intend to apply our lexicostatistical lists to earlier periods, we will attempt to make the members not only contemporaneously culture-free, but also as nearly culture-free throughout time as possible.

[3] Dialects as well as (distinct) languages can be QD classified. The usefulness of this classification depends entirely on the purpose to which it is put.

It has long been a conviction of dialect geographers that the best way of attacking the classification of dialects is via the vocabulary. At present the only competing type of classification is that based on phonetic (or phonemic) changes. There is no compulsory relation between the two types of classification. If two idiolects are classed together in one classification, it need not follow that they will be classed together in the other.

If dialects differ by a phonetic change, there is interference in communication between their respective speakers. If dialects differ by an item of basic vocabulary, there is likewise interference in communication between their respective speakers. There is no known way of regarding different phonetic changes as commensurate with each other so as to be able to measure the interference with communication which they cause. Items of basic vocabulary, to the extent that they are otherwise homogeneous, are commensurate, and thus suggest an ideal index of communicational interference between dialects. This interference can be regarded as the entropy of the system.

It therefore appears reasonable to index the interference in communication between dialects in terms of the frequency in the same meaning of non-cognate words. We consider only non-cognate words. If word D_{1i} in dialect D_1 is cognate with, and has the same meaning as, word D_{2i} in dialect D_2, then the meaning of D_{2i} can be inferred from D_{1i} by examining the results reached by applying the rules of phonemic transformation (= the rules of phonemic correspondence regarded as an operation). It is likewise reasonable to believe that the effects of analogy are normally not sufficient to impede this inference, and can therefore be disregarded. Thus cognateness is regarded as unaffected by analogic change.

If the average frequency of homosemantic non-cognate basic vocabulary is an index of the interference in communication between speakers of different dialects, then we might wish to seek whether in fact the progress of this interference is in any way correlated with time. The approach thus far has been empirical, and has met with just enough success to warrant further research, but not enough to put the matter beyond criticism.

If the resistance to replacement of a particular word in a particular meaning is the difference in frequency of that word in that meaning from the frequency of its nearest competitor, then to get a simple correlation with time we should wish to use meanings for which the ordinary words have in most cases the same resistance to replacement. The need for communication places a general limitation on the number of replacements that can occur relatively close in time in the same system. It is clear that all of our experience suggests that we should look at the system as being one in which each word is under a pressure for replacement and a pressure against it, but that the pressure for replacement (entropy) is sufficiently great so that it wins through at certain points.

We have reason to believe that the points of break-through are not entirely random. If words of high resistance are actually less likely to be replaced, then if the list chosen contains words of varied resistance, the break-through should normally occur more often in the words of less resistance. Over long periods, it would be increasingly likely that the same word would be replaced twice. If this factor is not taken into account in the calibration, an inevitable distortion will occur in the application of a relatively short-term calibration to a relatively long one.

Some who use lexicostatistics use the calibration established by Swadesh and Lees, but use different words. If there are differences in susceptibility to replacement among different words, the risk of error involved in this procedure is increased.

The possibility of multiple replacements increases the variety of the effects of a constant in different language continua and over different spans in the same continuum. For this reason, the study of replacements over shorter periods than a thousand years may provide results important for the establishment of the constant.

Among the words in the Swadesh diagnostic list, there are a large number with high resistance to replacement. This is demonstrable not on the basis of their relatively high frequency, but on the basis of their known persistence in control languages. Nevertheless, there are also included in the diagnostic list some large number of words which by the same token have a much lower probability of persistence.

It is interesting to note that in the Swadesh 100-word list (1955*b*), the range of variation in persistence values has been reduced (from approximately 1%–100% for the 200-word list) to 21%–100%, and that the concentration of persistence values is noticeably high at the upper end. This can be regarded as an improvement. Since we are, however, interested in variation, rather than in persistence, a more useful list would show fewer or no extremely stable items. Since we are furthermore interested in as nearly random as possible a distribution of the replacements, it would also show the characteristic of having a relatively large number of meanings concentrated over a relatively small percentage, say two or three decades.

Various lists could be used for special purposes. For example, one could be designed for the dialectological analysis of a language. Here one would prefer a very sensitive list, in which the words were relatively equal in relatively low resistance to replacement.

For sub-grouping, it might be desirable to have a number of lists of different degrees of resistance according to the precision desired. For the determina-

tion of the order of the most distant relationships particularly, the calibration of lists of the most resistant words would be highly desirable, whereas for those of closer relationships a more sensitive list would be more useful.

[4] The choice of range of persistence values would be different for the demonstration of relationship. The aim in a proof of relationship is to demonstrate the probable existence of a pair of cognate morphs in the languages being compared. Since a pair of cognate words is also a pair of cognate morph sequences, it is useful to examine the words of the language which probably have the highest resistance to replacement. For cognates should appear among these if anywhere among the words of the language. Among those languages the relationship of which is regarded as proved, it is unlikely that there is a pair of languages without a single pair of homosemantic cognates.

Swadesh has made it clear that it was one of his hopes to convert his lexicostatistical test, by itself, into an argument for relationship. However, to do so he must inevitably use a test other than systematic matching of phonemes as a criterion of a cognate-like relation. This he supplies by a rule of similarity (1954*a*: 319ff.) There is, however, no way of demonstrating that his particular rule of similarity is sufficient.

In fact, to use a lexicostatistical list in this way involves the investigation in the greatest risks, and is unnecessary when the languages themselves are accessible. The value of a comparison is greatest as the interval for which it is reliable grows. Because of linguistic change, however, the probability of recognizing cognates must decrease as the interval grows, even when entire languages are subjected to comparison. If the material examined by the comparatist is restricted to the entries of a diagnostic list, the chances of making a correct determination are greatly reduced. The list should therefore not be used alone in a proof of relationship. Since, however, the list does contain basic vocabulary, its contents can be used like any other part of the language which is more likely to exhibit a retention.

The use of a lexicostatistical list in the proof of relationship is thus different from its use in sub-grouping and in glottochronology. For proof of relationship we should be interested in the morph sequences of highest resistance. Since the proof of relationship depends on no more than the proof of the existence of a single pair of cognate morph sequences, the examination of the words of highest resistance is among the first steps to be taken by the comparatist.

By SARAH C. GUDSCHINSKY☆

[1] At the time I wrote 1956*b*, I saw only one generalized use for the techniques of glottochronology: to provide a time-depth estimate, with implications of sub-grouping as an incidental by-product. I would now like to register a change of viewpoint. As I now see it, there are at least three different uses of glottochronology, with at least some difference in technique required by each. Glottochronology can be used on raw data for the purpose of postulating possible relationships and degrees of relationship; it can be used following careful comparative study either for the purpose of estimating divergent time, or for establishing a sub-grouping of related languages. Each of these uses is discussed at some length by Hymes, but there are a few points that I would like to especially emphasize.

According to my present view, the first usefulness of a series of word lists is for the setting up of a skeleton classification to guide later research, i.e., the lists are compared and instances of greater than chance resemblance are assumed to imply genetic relationship. These resemblances are quantified (simple percentage at this point is fully as useful as divergence time, and undoubtedly less misleading); these quantified results permit a tentative grouping of the languages into sub-groupings with varying degrees of supposed relationship.

Such groupings in turn provide the outline for further exploration of the relationships, and for the detailed and painstaking work that is required for establishing relationships. The wide-ranging use of glottochronology facilitates field work by narrowing the range of search for relationships at a given level.

A point of particular emphasis is that sub-grouping and estimation of time depth are legitimate only *after* careful comparative work has proved the relationships, and only on the basis of proved cognates (i.e., cognates that are elements in reconstructions). This means that the use of "probable cognates" or guess at "glottochronological sames" should be limited to studies that are setting up hypotheses, and not used in the more exacting task of measuring time. This is not intended to imply that sloppy work is adequate for setting up the initial hypotheses. It would probably be generally agreed that the validity of such hypotheses is in direct proportion to the skill, experience, and—perhaps—the intuition of the investigator who proposes them.

At this point, however, I specifically reject the notion that the discovery of more resemblances than would be expected on the basis of chance constitutes in itself "proof" of genetic relationship; and I seriously question the more basic notion that all evidence of relationship is primarily statistical. There would be a great deal of disagreement, of course, as to how much of what kind of evidence constitutes complete proof. But I would consider a relatively small body of patterned similarity (i.e., material for reconstructing a plausible phonemic pattern and part of the grammatical structure) more weighty than a much larger number of unpatterned similarities.

Lamb's terminology seems useful for discussing this difference between proved relationship and postulated relationship (Sydney M. Lamb, "Some Proposals for Linguistic Taxonomy," *Anthropological Linguistics* [1959] 1:33–49). Relationships postulated on the basis of similarities, but not yet checked by comparative studies (or not checkable by comparative studies) would be Probable Relationships; those for which the comparative evidence seemed conclusive would be Established Relationships.

[2] It would seem to me that there is somewhere a happy medium between the repudiation of the "mesh principle" and its over-emphasis. Mesh relationships are a result, not an explanation. Presumably every shared linguistic trait that is not of chance duplicate origin is the result of either homogeneity at the time it originated, or of later diffusion from its place of origin. It is well known that it is possible to recover some of the history of a language by careful comparison of the linguistic evidence with what is known of the historical, geographic, and cultural factors. To dismiss all this under the cover term "mesh relationship" is to obscure potentially useful facts.

An example may be seen in my Mazatec studies (1955, 1958). The Mazatec dialects present a typical mesh, with the dialect of San Miguel in particular exhibiting some characteristics in common with the Jalapa dialect, and some in common with the Huautla dialect. This statement, however, says nothing about the history of the San Miguel dialect, nor how the mesh originated. A careful study of the linguistic data permits us to say that the San Miguel dialect was originally homogeneous with the Jalapa dialect, but that it later shifted to a close attachment to the Huautla dialect. A comparison of these facts with what is known of the local

history makes it possible to correlate these linguistic shifts with the political changes, and to "explain" the occurrence of this particular mesh.

[3] I suspect that the problems involved in filling out properly the diagnostic list may be sometimes greater than is usually realized. I would suggest, for example, that when Rae uses *garra* as the Spanish equivalent of *claw*, and Swadesh (supported by Taylor) uses *uña*, the problem begins with a basic difference in the referent for the English word *claw*. *If* these men speak the same dialect of Spanish that I do, Rae is thinking of *claw* as referring to the entire clawed foot, while Swadesh and Taylor think of *claw* as referring only to the animal analogue of a fingernail. This difference appears to occur in some dialects of English, as I have noted it among my colleagues in discussion of this item on the test list.

I have recently found in checking with colleagues that there are other words on the 200-word list for which we consistently use different areas of meaning. This means that in addition to the difficulties already reported in finding simple meanings in various languages, there is also the difficulty of being sure that each investigator is working with the same meaning.

In this connection, I might add that I think Taylor was too harsh in his judgment of Rae. Within the *campesino* dialect of Spanish which I learned in rural Mexico as a by-product of my study of Mazatec, Rae's "translation" of *pájaro* for *ave* is not only the most common, but probably the only, choice: his *quemar* for *arder, camino* for *vareda, pecho* for *seno,* and *pescozo* for *cuello* seem to me far more common. On the other hand, his *pequeño* for *chico* seems to me less common.

But there is something much deeper involved here than the simple question of whose variety of Spanish is more acceptable or more common. Certainly for all twelve of the words in which Rae's choice disagrees with Swadesh's, both choices are acceptable Spanish, and would be understood by all speakers at least of Mexican Spanish. But suppose that two lists were filled out in different parts of Mexico. And suppose that these hypothetical lists differed in every case where there is such a choice (i.e., a minimum of twelve differences). And suppose that each investigator involved were convinced that he had the commonest equivalent for the area or dialect that he was working with. Note that the difference between these two "dialects" would be nearly 600 years! And such a difference could be found in an almost completely homogeneous linguistic community! It should also be noted that the wide knowledge of Spanish that might safeguard us in considering Romance data (that caused Taylor to question the propriety of Rae's translations) is lacking in many of the American Indian languages in which we are currently using the lists.

All this leads to the question, "What does glottochronology measure when it gives diverge time?" Does it measure the point at which dialects began to diverge? Or the end of effectual communication? (No, this is not possible, since we can get divergence time for dialects that are still mutually intelligible.) Eventually, it seems to me, there needs to be some clarification of exactly what the time depths mean.

Along this same line, there is the question of what is meant by saying that in a group of divergence times for a single group of related languages, the oldest time is taken to be the more accurate. Is this intended to imply that successive splits are not possible?

[4] "It does not seem fair to conclude from a study so largely within this range that 'any hope of determining absolute time—by some improvement in the precision of lexico-statistical technique—seems to have been permanently lost.'"

This statement of mine should be read within the following context, ". . . since the order of differentiation can be deduced from the comparative material but the precise number of cognates lost and reborrowed cannot be so deduced."

The point is specifically that these may be changes in the basic vocabulary of such a nature that borrowed items are counted as glottochronological "sames" even by the most careful worker, and that in *such cases* there is no way of knowing that the items are borrowed, and hence no way of correcting the skewed time depth.

Hymes may be right, however, in assuming that such undiscoverable reborrowing is likely only in very closely related dialects such as the Mazatec under discussion.

By Shirō Hattori☆

[1] What I assume by proposing the replacement of 2 by 1.4 in the abovementioned formula, is that the factors of drift and dregs (these terms will be defined later) are always present even when that of contact is absent. Has the combination of these three not caused a too short glottochronological time depth for the Romance languages?

In my *The Affinity of Japanese* (1959), in a reprinting of a 1957 article of the same title, I discussed this point more in detail. I quote the discussion here, translating it from Japanese (pp. 199–202):

I will show a clear example of the independent similar development of related languages without mutual influences after separation.

Proto-Germanic *ā has been preserved as [a:] (and [e:]) in German, but has been changed into [u:] (and [i:]) in English. Proto-Germanic *ai has been changed into [ou] (and [i:]) in English, but into [e:] (and [i:]) in German. These are markedly divergent changes. On the other hand, both languages have dipthongized the Proto-Germanic long close vowels *ī, *ū into [ai] [au], and changed the Proto-Germanic long half-close vowels *$\bar{e}_2$, *ō into close [i:] [u:]. Moreover, it is to be noticed that these dipthongizations cannot be due to a mutual influence, because they took place during the periods between Middle English and Modern English, and between Middle High German and Modern High German. This is a surprising coincidence.

I thought that it would be of interest if we could establish that similar changes took place also in vocabulary, i.e., that both in English and in German the same items were independently replaced by cognate words. However, examples are apparently few, and I was able to find, out of Swadesh's 215-item list, only the following:

The meaning 'to hold' was expressed in Gothic by *haban* [which corresponds to Latin *capere* 'to seize, take'], and in OHG by *habēn*, which became *haben* 'to have' in ModHG. On the other hand, ModHG *halten* 'to hold' comes from OHG *haltan* 'to keep, guard, observe, pasture.' OE *habban* meaning 'to have, hold,' and OE *healdan* meaning 'to hold, keep, guard, pasture,' have become ModE *have* and *hold* respectively.

The meaning 'man (male person)' was expressed in Gothic generally by *wafr* [which corresponds to Latin *vir* 'man (male person'], and in OE generally by *wer*, but since that time, ME *man,* which meant originally 'person, human being,' took its place. In OHG the meaning 'man (male person)' was expressed by *man, goman, war,* but it is expressed by *Mann* in ModHG.

The meaning 'right (vs. left)' was expressed in OE by *swīþra*, in OHG by *zeso*, but it is expressed by *right* in ModE, and by *recht* in ModHG, which came from OE *riht* 'straight, just, etc.' and OHG *reht* 'straight, just, etc.,' respectively.

The meaning 'to wash' was expressed in OE by *þwēan* (parts of the body, dishes, etc.), *wæscan* (clothes, etc.), *swillan*. In ModE only *wash*, which came from OE. *wæscan*, has remained. OHG had *wascan* and *dwahan* (parts of the body, dishes, etc.), but only the former has remained as *waschen* in ModHG.

However, it is very difficult to find such examples, and we have to think that "the tendency of independent similar development of related languages after separation" must mean in the field of vocabulary rather 'the tendency of independent retention (and loss) of the same words.'

Anyhow, we have to examine empirically whether it is possible or not to use the formula $d = \log c/2 \log r$ [this will be done by using the formula $d = \log c/x \log r$, in reference to cases where we know d, c, and r], in order to contribute to the refinement of the method of glottochronology.

It should be borne in mind that two dialects that are already different in phonology, etc., tend to slow down the spread of divergent development, especially in vocabulary, when they are adjacent to each other. The shift of consonants of HG began before the middle of the eighth century, and was completed at the end of the same century. Nevertheless, the glottochronological time

depth between Dutch and ModHG is very small. Using Swadesh's old list, I got 82.2% (residue) common to ModHG and Dutch. This means:

316 years in $d = \log c/2 \log r$ (r:0.805)
451 years in $d = \log c/1.4 \log r$

On the other hand, the great difference of consonant systems of HG and Dutch began to appear more than 1,200 years ago. The two languages should have influenced each other in the development of lexicon, although they were different in phonology. We can see similar phenomena between the Japanese dialects of Kyoto and Tokyo, and between the Chinese dialects of Peking and Suchow.

Therefore, even if English and German began to diverge *circa* 2,000 years ago, we should not put the figure 2,000 for d in the above formula. Because, before the Anglo-Saxons came to England in the latter half of the fifth century, they lived in the neighborhood of the ancestors of the Germans, and they probably influenced each other linguistically. (In 1954 I used provisionally the figure 1,800 for d [without any guarantee], and got the figure 1.4 in place of 2.) I should like to do similar research on Norwegian and Icelandic.

I should like to call "drift and dregs" the above-mentioned tendency of independent replacement by cognates, loss (= drift), and retention (= dregs) of the same words in a group of related languages. I think the exploration of drift and dregs in this sense, if there are any, is very necessary for the refinement of the method of glottochronology.

[2] I have been trying for several years to enlarge Swadesh's list, in order to heighten, if possible, the statistical reliability. Another objective of mine was to compile a list that may be more effective for exploring the proofs of genetic relationship between languages.

To start with, I compiled a pretty large body of basic vocabulary, including all the items found in the following lexicons. Several scholars collaborated in this work.

KOKUSAI BUNKA SHINKŌKAI, *Basic vocabulary of Japanese*, 1944.

DOI, KŌCHI, Nippongo no sugata, 1943, pp. 333–37.

AIIKU KENKYŪJO, Linguistic development of children, 1943, pp. 30–53.

SAKAMOTO, ICHIRŌ, *Basic vocabulary of Japanese*, 1943.

RAXMANOV, I. V. (Ed.), *Slovar'—Minimum po anglizyskomu, nemetcskomu i francuzskomu yazykam*, 1951.

MUEGGE, M. A., *German word-frequency vocabulary*, 1936.

BEACH, T., *French word-frequency vocabulary and sentence construction book*, 1933.

FREI, H., *Le livre des deux mille phrases*, 1953.

Ministère de l'Éducation Nationale, *Le français élémentaire*, 1954.

JOSSELSON, H. H., *The Russian word count and frequency analysis of grammatical categories of standard literary Russian*, 1953. (First half.)

COHEN, M., *Questionnaire linguistique*, 1950–51.

BUCK, C. D., *A dictionary of selected synonyms in the principal Indo-European languages*, 1949.

PALMER, H. E., *1000 basic words of English*, 1952.

Out of this body, I selected, in collaboration with Mr. H. Kitamura, the most basic items, excluding the following:

(1) Too specific items, e.g., "eyelashes," "pupil," "canthus," but "eye" is included.

(2) Items with abstract meaning: "meaning," "courtesy," "temperament," "to feel," etc.

(3) Items peculiar only to a few nations or civilized peoples. However, "snow," "ice," etc., are included, because they are common to many nations.

(4) Items that are very possibly duplicated in many languages, e.g., "food" is included, but "meal," "dinner," are not; "to work" is included, but "a work," "a job," are not; "near" is included, but "to approach" is not; "to steal" is included, but "thief" is not, etc.

Although we found some of them inappropriate, we have included all the items of Swadesh's old and new lists, in order to make it possible to compare the obtained material with that obtained by other glottochronologists.

Before I started the work, I intended to compile a list of about 1,000 items. But it turned out to be very difficult to find more than 500 items. Our list that was provisionally published in August, 1957, has only 457 items.

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history makes it possible to correlate these linguistic shifts with the political changes, and to "explain" the occurrence of this particular mesh.

[3] I suspect that the problems involved in filling out properly the diagnostic list may be sometimes greater than is usually realized. I would suggest, for example, that when Rae uses *garra* as the Spanish equivalent of *claw*, and Swadesh (supported by Taylor) uses *uña*, the problem begins with a basic difference in the referent for the English word *claw*. *If* these men speak the same dialect of Spanish that I do, Rae is thinking of *claw* as referring to the entire clawed foot, while Swadesh and Taylor think of *claw* as referring only to the animal analogue of a fingernail. This difference appears to occur in some dialects of English, as I have noted it among my colleagues in discussion of this item on the test list.

I have recently found in checking with colleagues that there are other words on the 200-word list for which we consistently use different areas of meaning. This means that in addition to the difficulties already reported in finding simple meanings in various languages, there is also the difficulty of being sure that each investigator is working with the same meaning.

In this connection, I might add that I think Taylor was too harsh in his judgment of Rae. Within the *campesino* dialect of Spanish which I learned in rural Mexico as a by-product of my study of Mazatec, Rae's "translation" of *pájaro* for *ave* is not only the most common, but probably the only, choice; his *quemar* for *arder, camino* for *vareda, pecho* for *seno,* and *pescozo* for *cuello* seem to me far more common. On the other hand, his *pequeño* for *chico* seems to me less common.

But there is something much deeper involved here than the simple question of whose variety of Spanish is more acceptable or more common. Certainly for all twelve of the words in which Rae's choice disagrees with Swadesh's, both choices are acceptable Spanish, and would be understood by all speakers at least of Mexican Spanish. But suppose that two lists were filled out in different parts of Mexico. And suppose that these hypothetical lists differed in every case where there is such a choice (i.e., a minimum of twelve differences). And suppose that each investigator involved were convinced that he had the commonest equivalent for the area or dialect that he was working with. Note that the difference between these two "dialects" would be nearly 600 years! And such a difference could be found in an almost completely homogeneous linguistic community! It should also be noted that the wide knowledge of Spanish that might safeguard us in considering Romance data (that caused Taylor to question the propriety of Rae's translations) is lacking in many of the American Indian languages in which we are currently using the lists.

All this leads to the question, "What does glottochronology measure when it gives diverge time?" Does it measure the point at which dialects began to diverge? Or the end of effectual communication? (No, this is not possible, since we can get divergence time for dialects that are still mutually intelligible.) Eventually, it seems to me, there needs to be some clarification of exactly what the time depths mean.

Along this same line, there is the question of what is meant by saying that in a group of divergence times for a single group of related languages, the oldest time is taken to be the more accurate. Is this intended to imply that successive splits are not possible?

[4] "It does not seem fair to conclude from a study so largely within this range that 'any hope of determining absolute time—by some improvement in the precision of lexico-statistical technique—seems to have been permanently lost.'"

This statement of mine should be read within the following context, ". . . since the order of differentiation can be deduced from the comparative material but the precise number of cognates lost and reborrowed cannot be so deduced."

The point is specifically that these may be changes in the basic vocabulary of such a nature that borrowed items are counted as glottochronological "sames" even by the most careful worker, and that in *such cases* there is no way of knowing that the items are borrowed, and hence no way of correcting the skewed time depth.

Hymes may be right, however, in assuming that such undiscoverable reborrowing is likely only in very closely related dialects such as the Mazatec under discussion.

By Shirô Hattori☆

[1] What I assume by proposing the replacement of 2 by 1.4 in the abovementioned formula, is that the factors of drift and dregs (these terms will be defined later) are always present even when that of contact is absent. Has the combination of these three not caused a too short glottochronological time depth for the Romance languages?

In my *The Affinity of Japanese* (1959), in a reprinting of a 1957 article of the same title, I discussed this point more in detail. I quote the discussion here, translating it from Japanese (pp. 199–202):

I will show a clear example of the independent similar development of related languages without mutual influences after separation.

Proto-Germanic *ā has been preserved as [a:] (and [e:]) in German, but has been changed into [u:] (and [i:]) in English. Proto-Germanic *ai has been changed into [ou] (and [i:]) in English, but into [e:] (and [i:]) in German. These are markedly divergent changes. On the other hand, both languages have dipthongized the Proto-Germanic long close vowels *ī, *ū into [ai] [au], and changed the Proto-Germanic long half-close vowels *ē$_2$, *ō into close [i:] [u:]. Moreover, it is to be noticed that these dipthongizations cannot be due to a mutual influence, because they took place during the periods between Middle English and Modern English, and between Middle High German and Modern High German. This is a surprising coincidence.

I thought that it would be of interest if we could establish that similar changes took place also in vocabulary, i.e., that both in English and in German the same items were independently replaced by cognate words. However, examples are apparently few, and I was able to find, out of Swadesh's 215-item list, only the following:

The meaning 'to hold' was expressed in Gothic by *haban* [which corresponds to Latin *capere* 'to seize, take'], and in OHG by *habēn*, which became *haben* 'to have' in ModHG. On the other hand, ModHG *halten* 'to hold' comes from OHG *haltan* 'to keep, guard, observe, pasture.' OE *habban* meaning 'to have, hold,' and OE *healdan* meaning 'to hold, keep, guard, pasture,' have become ModE *have* and *hold* respectively.

The meaning 'man (male person)' was expressed in Gothic generally by *wafr* [which corresponds to Latin *vir* 'man (male person')], and in OE generally by *wer*, but since that time, ME *man*, which meant originally 'person, human being,' took its place. In OHG the meaning 'man (male person)' was expressed by *man, goman, war*, but it is expressed by *Mann* in ModHG.

The meaning 'right (vs. left)' was expressed in OE by *swiþra*, in OHG by *zeso*, but it is expressed by *right* in ModE, and by *recht* in ModHG, which came from OE *riht* 'straight, just, etc.' and OHG *reht* 'straight, just, etc.,' respectively.

The meaning 'to wash' was expressed in OE by *þwēan* (parts of the body, dishes, etc.), *wæscan* (clothes, etc.), *swillan*. In ModE only *wash*, which came from OE, *wæscan*, has remained. OHG had *wascan* and *dwahan* (parts of the body, dishes, etc.), but only the former has remained as *waschen* in ModHG.

However, it is very difficult to find such examples, and we have to think that "the tendency of independent similar development of related languages after separation" must mean in the field of vocabulary rather 'the tendency of independent retention (and loss) of the same words.'

Anyhow, we have to examine empirically whether it is possible or not to use the formula $d = \log c/2 \log r$ [this will be done by using the formula $d = \log c/x \log r$, in reference to cases where we know d, c, and r], in order to contribute to the refinement of the method of glottochronology.

It should be borne in mind that two dialects that are already different in phonology, etc., tend to slow down the spread of divergent development, especially in vocabulary, when they are adjacent to each other. The shift of consonants of HG began before the middle of the eighth century, and was completed at the end of the same century. Nevertheless, the glottochronological time

depth between Dutch and ModHG is very small. Using Swadesh's old list, I got 82.2% (residue) common to ModHG and Dutch. This means:

316 years in $d = \log c/2 \log r$ (r:0.805)
451 years in $d = \log c/1.4 \log r$

On the other hand, the great difference of consonant systems of HG and Dutch began to appear more than 1,200 years ago. The two languages should have influenced each other in the development of lexicon, although they were different in phonology. We can see similar phenomena between the Japanese dialects of Kyoto and Tokyo, and between the Chinese dialects of Peking and Suchow.

Therefore, even if English and German began to diverge *circa* 2,000 years ago, we should not put the figure 2,000 for d in the above formula. Because, before the Anglo-Saxons came to England in the latter half of the fifth century, they lived in the neighborhood of the ancestors of the Germans, and they probably influenced each other linguistically. (In 1954 I used provisionally the figure 1,800 for d [without any guarantee], and got the figure 1.4 in place of 2.) I should like to do similar research on Norwegian and Icelandic.

I should like to call "drift and dregs" the above-mentioned tendency of independent replacement by cognates, loss (= drift), and retention (= dregs) of the same words in a group of related languages. I think the exploration of drift and dregs in this sense, if there are any, is very necessary for the refinement of the method of glottochronology.

[2] I have been trying for several years to enlarge Swadesh's list, in order to heighten, if possible, the statistical reliability. Another objective of mine was to compile a list that may be more effective for exploring the proofs of genetic relationship between languages.

To start with, I compiled a pretty large body of basic vocabulary, including all the items found in the following lexicons. Several scholars collaborated in this work.

KOKUSAI BUNKA SHINKŌKAI, *Basic vocabulary of Japanese*, 1944.

DOI, KŌCHI, Nippongo no sugata, 1943, pp. 333–37.

AIKU KENKYŪJO, Linguistic development of children, 1943, pp. 30–53.

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FREI, H., *Le livre des deux mille phrases*, 1953.

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BUCK, C. D., *A dictionary of selected synonyms in the principal Indo-European languages*, 1949.

PALMER, H. E., *1000 basic words of English*, 1952.

Out of this body, I selected, in collaboration with Mr. H. Kitamura, the most basic items, excluding the following:

(1) Too specific items, e.g., "eyelashes," "pupil," "canthus," but "eye" is included.

(2) Items with abstract meaning: "meaning," "courtesy," "temperament," "to feel," etc.

(3) Items peculiar only to a few nations or civilized peoples. However, "snow," "ice," etc., are included, because they are common to many nations.

(4) Items that are very possibly duplicated in many languages, e.g., "food" is included, but "meal," "dinner," are not; "to work" is included, but "a work," "a job," are not; "near" is included, but "to approach" is not; "to steal" is included, but "thief" is not, etc.

Although we found some of them inappropriate, we have included all the items of Swadesh's old and new lists, in order to make it possible to compare the obtained material with that obtained by other glottochronologists.

Before I started the work, I intended to compile a list of about 1,000 items. But it turned out to be very difficult to find more than 500 items. Our list that was provisionally published[3] in August, 1957, has only 457 items.

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Twenty Years of Peyote Studies

by Weston La Barre

PEYOTE (from the Aztec *peyotl*) is a small, spineless, carrot-shaped cactus, *Lophophora williamsii* Lemaire, which grows wild in the Rio Grande Valley and southward. It is mostly subterranean, and only the grayish-green pincushion-like top appears above ground, with spiral radial grooves dividing the puffy prominences which bear linearly-spaced tufts of fine gray-white flocculence, somewhat like artists' camels-hair paintbrushes. Cut off horizontally about ground level, and dried into a hard woody disc, this top becomes the so-called "peyote button"—often called "mescal button," confusingly since it does not come from the non-cactus succulent, the mescal proper, from whose fermented sap, pulque, the brandy mescal is distilled; also, erroneously, called "mescal bean" which is the Red Bean, *Sophora secundiflora* (ortega) Lag. ex DC; and, further, once quite mistakenly identified with the Aztec narcotic mushroom *teonanacatl*, a Basidiomycete, a true member of the Fungi. Nine psychotropic alkaloids, an unusual number even for a cactus, are contained in natural pan-peyotl; some of these are strychnine-like pharmacodynamically, others (notably mescaline) hallucinogenic. For this reason the psychotomimetic mescaline has been experimentally investigated in recent psychiatric research, along with its fellow-indoles such as lysergic acid; and for this reason, its hallucinogenic qualities, American Indians have used pan-peyotl in native "doctoring," witchcraft, and religious rituals. As a religious cult, peyotism is pre-Columbian in Mexico. Toward the end of the nineteenth century, peyotism spread, via Texan tribes and Athapaskans of the Southwest, to the Indians of the United States, mostly following the subsidence of the Ghost Dance, for which it largely substituted, now as a peaceful intertribal nativistic religion, in places somewhat acculturated to Christianity. It is now the major religious cult of most Indians of the United States between the Rocky Mountains and the Mississippi (including the remnants of eastern Algonkin tribes and the Siouan Winnebago), and additionally in parts of southern Canada, the Great Basin, and east-central California. The appeal of peyote is based upon the visions it induces, *viz.* its "medicine power," and its availability therefore in native doctoring is culturally based upon the aboriginal vision quest and the religious and ideological premises of this quest. Peyote is generally agreed by experts to be non-habit-forming; it is non-soporific and not, therefore, technically a "narcotic."

Some twenty years ago the present writer sought to summarize all that was then known about peyote (*Lophophora williamsii*)—its botany, ethnology and history, chemistry, psychology and physiology—as well as about the "mescal" bean (*Sophora secundiflora*) and the narcotic mushroom *teonanacatl*, both of which had been confused with peyote. This summary (La Barre 1938) was based upon the extensive published literature on peyote and peyotism, on the generous loan of current and unpublished field notes by many persons, and on the writer's own field trips during several years to fifteen peyote-using American Indian tribes.

In the subsequent two decades, peyotism has remained a lively subject of Americanist research. Despite the standardization of the rite, a number of new substantive details have been added to our knowledge. Peyotism has spread to several new tribes. Medico-psychiatric research on mescaline, the principal hallucinogenic alkaloid of the nine in natural panpeyotl, has progressed considerably beyond its status in 1938. Certain problems concerning the origin and diffusion of peyote have been argued and perhaps clarified. And, finally, important new problem-oriented and methodological studies have been made on the basis of peyote

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LA BARRE began his studies of American Indians and peyotism with field work among the Kiowa and fourteen other North American tribes in 1935 and 1936. His *The Peyote Cult* is now being reprinted by The Shoe String Press (New Haven, Conn.)

The present article, submitted on September 11, 1958, was the first to receive CA☆ treatment (see inside front cover). It was sent to thirteen scholars. Substantive comments were returned by David F. Aberle, Donald Collier, Åke Hultkrantz, Wilhelm Koppers, Marvin K. Opler, Louise Spindler, George Spindler, and Anthony F. C. Wallace. Those contributions that were incorporated by the author into text or notes are indicated by a star (☆).

LOCATION OF TRIBES AT TIME OF PROBABLE FIRST CONTACT WITH PEYOTISM

data, which studies, not the least being the new and valuable materials on peyote music, are of wider general interest. It would probably be useful at the present time to summarize these studies for the general ethnologist and to attempt a perspective on the past as well as a prospect for future studies.

GENERAL WORKS

Of general works, the first to be mentioned is Gusinde's "Der Peyote-Kult" (1939), judiciously reviewed by Marvin Opler (1940*b*). Opler was critical of some *Kulturkreis* aspects of Gusinde's work and contrasted it methodologically with the Yale study. Despite the different approaches, Opler pointed out that, "Gusinde adds confirmation to La Barre's Yale University publication with substantial, though undeclared, agreement on most essential points" (p. 667). The most important of these, perhaps, concerns the early origins of peyotism in the United States, since Gusinde and La Barre worked entirely independently of one another and were ignorant of one another's researches. Opler states (p. 669) that, for Gusinde, the

> Carrizo, Tonkawa, Lipan, and Mescalero are on the direct line of diffusion from northeastern Mexico. The Mescalero are seen as the link to the Kiowa and Comanche, and the latter provide the connection to the Caddo, Delaware, southern Cheyenne and Arapaho, and finally to the Ute and Shoshoni.

It is, in fact, to the later published field work of both the brothers Opler that we owe the clarification of early diffusion of peyote to Texas and the eastern Southwest, a point on which Gusinde, La Barre, the Oplers, and most other students except Slotkin are in agreement. The major new contributions to our knowledge have been made at the other end of history, in respect of the modern diffusion of peyote to the Menomini, Navaho, Ute, and Washo. Slotkin has introduced alternative views concerning the diffusion of peyotism through the Hopi, Taos, and the Caddo, but specialists on these areas have questioned both his data and his conclusions, and his must be regarded as a minority viewpoint.

Slotkin's major work on peyote, *The Peyote Religion* (1956*a*), is extremely valuable for its summary of the present legal status of peyotism in state and federal laws (pp. 54–56), for its discussion of the local organizations and officers of the Native American Church (pp. 57–64), and for its excellent "Bibliography on Peyotism North of the Rio Grande, 1850–1955" (pp. 143–87). This bibliography of more than 550 items contains only 55 already cited by La Barre, and over 300 before and 77 since 1937 not in La Barre, so that the bibliographies largely supplement one another and together account for over a thousand items. As will be noted later, Slotkin was also responsible for the publication of a number of colonial documents, though some of these have been disputed as referring to peyote. In addition, he published a number of documents on the Native American Church, known to but only summarized in La Barre. Slotkin's *The Peyote Religion* must be regarded as the major source on the Native American Church, of which Slotkin was an officer. The real value of Slotkin's original contributions and documentations can scarcely be disputed, though his manner of presentation has been

criticized (La Barre 1957*b*) and also some of his conclusions (Beaver 1952).

The other extended works on peyotism include works by Aberle and Stewart on Navaho and Ute peyotism, Stewart on Washo-Northern Paiute, Slotkin and McAllester on Menomini, McAllester on peyote music, and the Spindlers on the place of peyotism in Menomini acculturation. These studies will be discussed below. Another general work is "La Magia del Peyotl" (Aguirre Beltrán 1952), an excellent though brief study, containing new material on the uses of peyote in colonial Mexico. "El Peyote al Traves de los Siglos" (Hijar y Haro 1937) is interesting for its somewhat standard bibliography placed in chronological order. Leonard (1942) has published documents indicating that in 1620 the Inquisition prohibited the use of peyote in Mexico, where it had been used for detecting thefts and for divination and prognostication. The distinguished botanist Schultes believes, on the basis of information recorded by Sahagun, that *Lophophora williamsii* has been used as a religious sacrament since 300 B.C., hence has been an item in economic botany for over two thousand years, and, on the basis of information in B. P. Reko, that it has been used as far south as Yucatan (Schultes 1938*b*). Schultes points out also that peyote was a problem to missionaries in Texas in 1760, and thinks that peyote came to the Kiowa Reservation earlier than is now assumed, though he has not demonstrated that the Kiowa were in that position at that date. Underhill has written a good summary, "Peyote" (1952), based on standard sources. She has also sharpened our awareness that the slight ritualization of the "peyote journey" of Plains tribes may have been influenced by the more developed ritual journeys in Mexico and the Southwest (1954: 649):

> A trait of Huichol and Tarahumara which did not reach the north until later times is the use of peyote. Connected with it is the ceremonial journey, with restrictions and special language. A similar journey is found among the Papago, where the object is not peyote but salt. Perhaps the salt journeys of both Hopi and Zuni may be faint echoes of it, as also the Taos camp at Blue Lake. The warpath behavior of the Chiricahua Apache with its restrictions and its special language . . . may be another echo, perhaps learned from the Papago who were neighbors and enemies.

However, we believe that such an influence on the ritualization of the peyote journey must have come through Apache groups in the Southwest and Texas, rather than through the Pueblo groups, though this adds an interesting sidenote to the standard theory of the diffusion of peyote.

LEGAL STATUS OF PEYOTISM

As earlier noted, Slotkin's work is the standard one on the legal status of peyotism and the Native American Church. Stewart added some interesting new data in his spirited argument against a Colorado state anti-peyote law (1956*a*). He notes that the Native American Church of Saskatchewan was chartered on November 3, 1954, and that a new group appeared legally in North Dakota on January 9, 1954. Twelve states have now issued charters to the Native American Church. The Twelfth Annual Meeting of the Church was held at Scottsbluff, Nebraska, from June 28 to July 1, 1956.

The use of peyote has been a burning legal issue especially among the Navaho. In a newspaper article datelined from Window Rock, Arizona (*New York Times* 1954), it was stated that thirteen members of the Native American Church had been jailed by the Navaho tribal leaders, and that Texas, Arizona, and New Mexico now prohibit the transportation and sale of peyote; nevertheless, the buttons, which cost ten to eleven dollars per thousand in Texas, were commonly available on the Navaho Reservation at five to ten cents each.

Missionaries have continued their determined hostility toward peyotism.[1] Niedhammer, in a "Statement on Peyote" (n.d.) prepared for his ecclesiastical superiors at the Saint Labre Indian Mission to the Cheyenne of Tongue River Reservation, condemned the use of peyote by Indians. Based on this document was an article (Scully 1941) which ended in the promulgation of the dictum that there is a complete incompatibility between peyotism and the Catholic religion. Curiously, an American Medical Association committee urged in its report of November 25, 1948, to the U.S. Secretary of the Interior, that the use of peyote be nationally outlawed as a habit-forming drug (Associated Press 1948; see also Braasch, Branton, and Chesley 1949). We can only conclude that these doctors did not base their opinion on adequate medical evidence. Certainly ethnologists who have used peyote repeatedly and have observed in circumstantial detail its use among Indians, in both cases without such assumed effects, quite uniformly agree that peyote is not habit-forming. This was asserted in a "Statement on Peyote" signed by La Barre, McAllester, Slotkin, Stewart, and Tax (La Barre *et al.* 1951). This document is in essential agreement with an earlier series of standard statements by Boas, Kroeber, Hrdlička, J. P. and M. R. Harrington, La Barre, Petrullo, Schultes, Elna Smith, and Osage Chief Fred Lookout against the (Chavez) Senate Bill 1399 of February 8, 1937. Although the final opinion must necessarily be a medical one, informed anthropologists are firmly united in their judgment and will doubtless continue to protest the neglect of medical evidence in the formulation of medical opinion (*Documents on Peyote* 1937).

PSYCHIATRIC RESEARCH

Psychiatric research on mescaline has continued to increase in recent decades. Claude and Ey (1934) reported on mescaline as an hallucinogenic substance; and Freedman, Aghajanian, Ornitz, and Rosner (1958), on the patterns of tolerance of lysergic acid and mescaline in rats. Denber and Merlis (1956*a*) studied the action of mescaline on brain-wave patterns in schizophrenics before and after administering Electric Shock Therapy, the antagonism between mescaline and Chlorpromazine (1956*b*), and also wrote on the therapeutic implications of mescaline-induced states (1954). Merlis and Hunter (1954) published on the effects of administration of mescaline to schizophrenics after Electric Shock Therapy; and Denber (1955), on its action in epileptics. Guttmann (1936), writing on artificial psychoses produced by mescaline, emphasized the paranoid states

that occasionally accompany mescaline intoxication which have been observed among both Indian (Radin 1926) and White subjects. The same authority was co-author of a study of mescaline and depersonalization (Guttmann and Maclay 1936) that reported research at Maudsley Hospital, London, and that stated that, "Mescalin [*sic*] depersonalization is identical with this symptom in morbid states, and therefore can be used as a model for therapeutic experiments" (p. 203). Himwich gives the formulas of various neurohormones, psychotomimetic agents, and tranquilizing drugs, including mescaline, in his research on drugs (1958), and had earlier shown that mescaline is psychotomimetic and that azacyclonol could suppress its effects (Himwich in Cholden 1956). Hoch (1952) and Kant (1931) also worked on the experimental induction of psychoses by mescaline. Abram Hoffer had experimented with mescaline for over three years by 1954, in his research on schizophrenia at the University of Saskatchewan—work supported, like Guttmann and Maclay's, by a grant from the Rockefeller Foundation. Lindemann and Malamud (1933) have made experimental analyses of the psychopathological effects of intoxicating drugs, including mescaline. De Ropp's book, *Drugs and the Mind* (1957), has a chapter on "The Mind and Mescaline" (pp. 27–60); he uses mostly European sources of earlier date, but cites the Statement, earlier mentioned, by American ethnologists in *Science*. Slotta and Szyszka (1933), working in São Paulo, Brazil, have reported new discoveries concerning mescaline. Wallace (1959) considers that response to mescaline intoxication depends very considerably on the cultural and situational milieu, as well as on individual personality. Wertham has twice published on mescaline and pain (1952*a*, 1952*b*). Wikler (1957) has summarized recent psychiatric and pharmacological work on mescaline.[2] This selection of psychiatric researches on mescaline does not pretend to be exhaustive of the relevant copious modern studies on psychopharmacology, but it is believed that it constitutes a representative sampling.

SPECIAL PROBLEMS

Basing his argument exclusively on textual evidence from colonial documents, the present writer early argued against Safford's facile identification of peyote and the Aztec narcotic *teonanacatl*, since the latter was always specifically identified as a narcotic mushroom (La Barre 1938, Appendix 3: "Peyote and Teo-Nanacatl," pp. 128–30). The final solution of the problem could, of course, rest only on properly botanical evidence. Subsequently the botanist Schultes rediscovered a narcotic *Basidiomycete* in Mexico, which he identified with the Aztec mushroom *teonanacatl*, reporting first in a botanical publication (Schultes 1939) and later in an anthropological journal (Schultes 1940*b*). The provocative, lavishly expensive ($125.00), and somewhat inaccessible work on mushrooms, including *teonanacatl*, by a wealthy amateur and J. P. Morgan partner (Wasson and Wasson 1957; Wasson 1956), is a rediscovery of the Aztec narcotic mushroom. Although in agreement on the botany and the ethnology of *teonanacatl*, these authors disagree upon the etymology of the word. La Barre had questioned Benvento's etymology, "bread of the gods," which was accepted by Safford and others, but Schultes (1940*b*) cites Simeón and V. A. Reko (as later did Wasson) to support the earlier etymology. The present writer, although now a minority of one, still retains his skepticism regarding this point in Safford also, and further points out that this question will ultimately be settled only by linguistic specialists. It is gratifying, incidentally, that among professional anthropologists the misleading term "mescal bean" as applied to *Lophophora williamsii* has been dropped, though still retained by British literary writers; there is no such listing in the latest *General Index* of the American Anthropological Association publications ([1951] 53: 37). Schultes has also written interestingly on the aboriginal therapeutic uses of *Lophophora williamsii* (Schultes 1940*a*).

Another disagreement, this time between ethnologists, has occurred over the relationship of peyotism to "mescalism," the former cult dealing with *Lophophora williamsii* (a cactus) and the latter with the "red bean" (*Sophora secundiflora*, a true member of the FABACEAE or Bean Family). The earlier Red Bean Cult was found among the Apache, Comanche, Delaware, Iowa, Kansa, Omaha, Osage, Oto, Pawnee, Ponca, Tonkawa, and Wichita, according to Howard (1957). This evidence was contained in La Barre's original dissertation, now on deposit in the Sterling Library at Yale University, but was condensed in the final published account (1938) which merely distinguished peyote and the "mescal bean" botanically and suggested a "Red Bean Cult" that may have preceded peyotism in Texas and the Plains. On the basis of "mescal" evidence, however, Howard regarded with skepticism the usual derivation of the peyote cult from Mexico via the Apache. Howard argued that the similarities of peyotism with the mescal bean cult indicate a derivation of the ritual content of peyotism from mescalism, and that the influences on ritual form would seem to be from north to south, contrary to the usual view. The present writer has replied to this by arguing (1) that the history and ethnology of peyotism proper already establish sufficiently a southern origin from Mexico via the Apache and other tribes of Texas and the eastern Southwest; (2) that the supposed similarities in ritual are limited, non-specific, and ambiguous; (3) that the botanical provenience of both *Lophophora* and *Sophora* are southern; and (4) that there are archaeological evidences of early date for the use of *Sophora* in southwest Texas (La Barre 1957*a*). This last argument has elicited further data from an archaeologist indicating abundant finds of *Sophora* in Texas sites (Campbell 1958). The present writer still looks to the south for the origins of both mescalism and peyotism, maintaining that the Plains Siouan and Algonkian mescalists who *late* and *historically* received peyote from southern tribes could hardly have shaped the original southern rite—even though an attenuated and earlier mescalism had come to them, *also* from the south. What we should look for is an earlier Red Bean Cult in Texas, among Apache tribes, and in Mexico, which, on this time level, might indeed have influenced the ritual content of Apache-Kiowa-Comanche peyotism, though this is still to be demonstrated.

NEW SUBSTANTIVE DATA ON THE PEYOTE RITE

Although the Plains rite is highly standardized and has been voluminously reported in tribal monographs since the classic account of Kroeber for the Arapaho (1907) and of Mooney for the southern Plains (1896), and although peyotism itself has since been monographed by La Barre, Gusinde, and Slotkin, there still remain a number of contributions to substantive detail. Howard has written on a Comanche spearpoint used in a Kiowa-Comanche ceremonial (1950). This was an item in the ritual paraphernalia of the road chief Levi Whitebear (a half-Negro, quarter-Kiowa, quarter-Comanche of Lincoln, Nebraska) to symbolize the old way of life in which hunting was important and as a token of thanksgiving. The spearpoint was placed, in order from the "moon," eagle wingbone whistle, spearpoint, staff. Howard regards this as "an intrusive element of recent origin" (1950: 5), but has since pointed out in correspondence that several other "fireplaces" use it, most of them apparently of Kiowa or Comanche origin, and that while he earlier believed it to be an otherwise functionless fetish, he has found it used in doctoring for bloodletting. Howard has also written about a Tonkawa peyote legend (1951); this is the standard and familiar stress-origin legend of peyote, except for the etiological rationalization of why the Tonkawa are called "cannibals" by other Indians. He has also described the "fireplace" of the Oto leader, Charles Whitehorn, since 1946 in the keeping of an Omaha, George Phillips; it is said to have been obtained by Whitehorn in a vision, but it is indistinguishable from the standard Kiowa-Comanche altar except for a heart under the fire and a transverse line from horn to horn of the "moon" (1956). Indeed, it is similar to an earlier Caddo-Delaware moon (La Barre 1938: 75, Fig. 4*b*)—a measure of the "originality" to be expected in peyote rites.

Probably the most significant recent contribution to our knowledge of early peyotism is that of Brant. In his study of peyotism among the Kiowa Apache (1950), he adds the valuable new historical detail that a Mescalero or Lipan Apache named Nayokogał brought peyotism to the Kiowa Apache about 1875. Since the Kiowa themselves obtained peyote about 1880, it may very well be that the Kiowa Apache were a link between the Kiowa and the other Apache tribes of Mexico and the Southwest. Brant's data are in any case entirely consistent with Morris Opler's southern Athapaskan ethnography and La Barre's southern Plains data on early peyotism. Brant adds another interesting detail that the Kiowa Apache ritual breakfast sometimes consists of pemmican and corn gruel. Since the original Mexican rite had boneless deer meat and parched corn in sugar-water—and some much later northern rites had canned corn beef and Cracker Jack—one can only remark that *plus ça change, plus c'est la même chose!*

The Cheyenne and Menomini, like the Winnebago, have long been known to celebrate a somewhat Christianized version of the old aboriginal rite. Spindler, in a preliminary paper on his Menomini studies (1951), adds the confirmatory details that the tepee poles represent Jesus and his disciples; the leader's staff is carved with crosses, and the leader makes the sign of the cross; the ashes are shaped into the form of a "dove"; and there is a small pedestal on the moon for the "Master" Peyote—some of these apparently derivative from Christianity. "But," he adds, "the basic concepts and premises of the cult are native-oriented though modified and perverted to meet the unique needs of the participants." The same author states that the Menomini got peyote from the Potowatomi via J. M. Mitchell in 1914, and were influenced by the Winnebago, though they rejected some of John Rave's Christian elements; the purposes of meetings were "salvation" and prophetic visions, and one old woman kept a supply of peyote on hand to "get a vision for a design" for her beadwork when she tired of the old designs—which might be compared with the older Menomini pattern for song- and design-getting (Spindler 1950).

Other new data include those of Tax on Fox peyotism (1955). The volume *Iowa* (1949: 473–74), in the "State Guide Series" also indicates that the Sac and Fox of Iowa have obtained peyotism and now get their buttons direct from Texas. As reported in Canadian newspapers, peyote has now spread to the Saulteaux of Manitoba near Fort Qu'Apelle and Portage la Prairie; and, as reported in early December of 1956, peyote was spreading in northern Saskatchewan and Alberta as well.

Malouf indicates in his study of Gosiute peyotism (1942) some of the problems of establishing dates. La Barre had placed the origin in 1921 and Hayes in 1925 (Hayes 1940) when a Sioux, Sam Lone Bear, brought to the Gosiute the "Western Slope Way" (which eschewed tobacco), although the orthodox "Tipi Way" supplanted this in a short time. According to Hayes there were less than a dozen users before 1925; Malouf says his informants vaguely estimated that it got established sometime between 1925 and 1928. These data, not really inconsistent with one another, raise the question whether one should indicate the introduction or the *floruit* of the cult in stating origins. Indeed, Hayes mentions that Gray Horse, a Washo from Fallon, Nevada, and a great leader in the western Basin, had in 1940 used Peyote for twenty-five years, that is, since 1915. Malouf considers that Sam Lone Bear (or Roan Bear) may be the Ute Ralph Kochampaniskin or "Lone Bear" who had held Washo meetings as early as 1932. Since M. K. Opler ☆ agrees with Malouf's interpretation of the Ute origin of Sam Loan Bear, and since Aberle and Stewart (1957) have definitively discussed the question, the identification as "Sioux" by La Barre's informant must now be regarded as superseded. In any case, the Washo and the Gosiute have now an entirely standard Plains rite.

The Navaho have a long history of factionalism, in part arising over peyotism. Kirk reports that peyotism was strenuously resisted around 1932. The Charter of Incorporation in New Mexico of the Native American Church is dated July 15, 1945, with respect to Navaho peyotists. But in 1947 there were five or six thousand participants, that is, 35% of the population in the Shiprock region, which accounts for one-third of the area of the Navaho Reservation (Kirk 1947). Further discussion of the Navaho and peyote will be deferred until we deal

below with the able monograph treatment of Aberle and Stewart on Ute-Navaho peyotism.

PROBLEMS OF DIFFUSION

Morris Opler has contributed a valuable description of a Tonkawa peyote meeting held in 1902 (1939*a*). Obtaining what are probably the last ethnographic materials available from this group, he states that the Tonkawa were taught peyotism by the Carrizo, and that a Chiricahua Apache visited the Tonkawa meeting of 1902. These data are consistent with those cited in Opler's standard and authoritative review of *The Peyote Cult* (1939*b*) that peyotism came to the Lipan and Tonkawa via the Tamaulipecan-Carrizo tribes; that the Mescalero received peyote before 1870; and that the Kiowa-Comanche got it between 1870 and 1880. Opler has also written on the use of Peyote by the Carrizo and the Lipan Apache. His description of a Carrizo peyote meeting, which included shamanistic tricks, indicates diffusion to the Tonkawa and Lipan Apache as well (1938). The material culture of Carrizo peyotism fits the later rite well, the shaman's bow pointing backward to the Mexican first-fruits hunting rite (as also does the Comanche spearpoint mentioned by Howard?) and forward to the leader's staff in the standard Plains rite. Opler mentions also that for these tribes the "male" peyote blossoms red and the "female" white, a belief that fits well transitionally between the notions of plant sexuality held in Mexico and the southern Plains belief that Peyote Woman can be heard singing when one has eaten a female peyote button.

Some special problems of diffusion are discussed in Merriam and D'Azevedo's study of Washo peyote songs (1957). In 1957 Washo peyotism was scarcely more than twenty years old, having been introduced by the Washo, Ben Lancaster, who had lived among the eastern and southern tribes. He began proselytizing in 1936, and by 1939 had obtained an enthusiastic group of Washo and Paiute adherents. But in 1940 his meeting was largely defunct; he built an octagonal church (of Osage origin?) and presided over the remnants. The reasons for the decline were partly the clash of conflicting ritual "ways" and partly the conflict of peyotist theology with the spirit-guidance concepts of the Washo; possibly, too, the rivalry between the old shamans and the new peyotists was involved.

The most thorough studies on the diffusion of peyotism in single tribes have been those of the Spindlers on Menomini, and of Aberle and Stewart on Navaho and Ute. In a paper on "Male and Female Adaptations in Culture Change," Louise and George Spindler (1958) introduced a new dimension into peyotist studies with their discussions of differential diffusion and acculturation, and then elaborated in their later studies.[3] Louise Spindler also discussed the problems of peyote and witchcraft in Menomini acculturation (1952), and at the same date George Spindler and Walter Goldschmidt (1952) published a preliminary programmatic discussion of the method Spindler was to use in his 1955 doctoral thesis. In this they discussed the problem of sociological and psychological variables among the Menomini and presented a graph on which the levels of socio-economic status of groups varied vertically and the degree of acculturation horizontally.

In *Sociocultural and Psychological Processes in Menomini Acculturation* (1955), George Spindler produced at the same time one of the ablest papers on acculturation and one of the most minutely researched documents on projective techniques in the culture-and-personality field. Spindler's problem was to study the differential acculturation of each of five discernible groups of individuals among the Menomini; his method was to make extensive Rorschach-test samples of individuals in each of these five groups. Members of the Menomini native-oriented Dream Dance group were the least acculturated and were roughly similar in personality structure to their Algonkian relatives, the Ojibwa. The Peyote Cultists, with an intermediate Kiowa-Comanche and Winnebago Christianized version of the cult, showed a systematic deviation from the other Menomini groups, based on their identification with the closely knit cult group of peyotists. The Transitionals, with both native and White culture experiences, the lower-class acculturated, and the middle-class acculturated were the other groups, the last being at the opposite extreme of a continuum from the Dream Dance group and showing basic personality reformulation. Spindler has shown precisely the context of the Menomini peyotists (p. 207):

> The systematic deviation in psychological processes demonstrated for the Peyote Cult consists of a relatively high degree of self-projective fantasy in a setting of anxiety, conflict, awareness, and introspection. This is accompanied by relative looseness of affect control and a possible decrement in reality control. This systematic deviation is represented statistically by consistent difference between the Peyotists as a group and all other Menomini categories, but the characteristics last named are shared with the transitionals.

The work of Aberle and Stewart on the diffusion of peyotism from the Ute to the Navaho is also admirable methodologically (1957). As early as 1954 Kluckhohn stated that, "Aberle and Moore in their studies of Navaho peyote use have also employed random sampling" (1954: 691). Kluckhohn and Leighton had noted in 1946 that, "The peyote and certain other religious cults flourish here" among the Navaho of Shiprock (1946: 125), long a center of anti-White feeling, and Thompson had included the Navaho in her discussion of the problems of acculturation in various tribes (1948). Aberle and Stewart attacked the problem systematically and statistically, giving careful attention to both geographic and psychological details. Despite continuous opposition from the Navaho tribal council, peyotism spread, in the authors' opinion, with respect to communications and geographic availability, although they regard the disgruntlement arising from the stock-reduction campaign as an important additional impetus to its spread. The present writer has reviewed this work elsewhere (1958) and will not deal with it further here; Morris Opler (1958) has also reviewed the work of Aberle and Stewart.

The same problem of differential diffusion has preoccupied other students, among them the distinguished sociologists Lasswell, Barber, and Shonle. Lasswell wrote on "Collective Autism as a Consequence of Cultural Contact: Notes on Religious Training and the Peyote Cult at Taos" (1935). In the opinion of the present writer, however, Lasswell attended too little to com-

parative ethnography and has perhaps been misled in his interpretations within this narrow framework of reference that he has chosen; but a historically longitudinal study of peyotism at Taos since colonial times is still worth doing. Barber points out the close temporal succession of Peyotism upon the Ghost Dance, and discusses John Wilson and the Kiowa shaman Baigya, both of whom bridged the two phenomena in time (1941). Barber concludes (pp. 674–75) that:

> The Ghost Dance and the Peyote cult, then, may in part be understood as alternative responses to a similar socio-cultural constellation. As such a response, the Peyote cult performs certain adaptive functions. On those whom it honors with leadership, it bestows prestige and status, serving as a path to social advancement. Public confession of sins in Peyote ceremonies is at once a mechanism for the dissolution of individual anxieties and a mode of social control. Like the old buffalo societies of the Teton Sioux, the cult can become a focus of tribal ceremonial and social activity. This interpretation, however, does not pretend to exhaust the possible understanding of the phenomena. It does not preclude the necessity for understanding the particular cultural patterns to which peyotism diffused and tracing their influence in the process of its assimilation. It does indicate the socio-cultural situation from which the Peyote cult was precipitated.

The usual psychological-ethnological explanation for the spread of peyote has been that of Shonle, who pointed out that peyote was diffusing in the same regions that had the old Plains vision quest (1925) and, indeed, the subsequent spread of peyotism has largely confirmed Miss Shonle's predictions. Barber writes (1941: 675) that:

> There may or may not be some relation between the importance of the vision in Plains culture and the Peyote cult. Shonle, for example, thinks there is. Petrullo [1934] criticizes the theory that Peyote is a substitute for the fasting and self-torture employed by the Plains Indians seeking a vision. I should say that the vision is an important element in the culture to which Peyote was assimilated, and, as such, exerted its influence, but that this cannot explain the particular occasion of the widespread diffusion of Peyote.

The present writer would point out in defense of Shonle that even among the Caddo-Delaware whom Petrullo studied, the peyote meeting was in the context of an ordeal or an endurance contest; that despite their closeness to the source of peyote, the Pueblos (with the exception of Taos, the most "Plains-like" of the Pueblos) have not accepted peyotism; that even at Taos peyotism has long had a difficult time; and, finally, that peyotism is still a very controversial subject among the Pueblo-influenced Navaho, despite the latter's common origin with the Apache, who were the major vehicle for the Mexico-Plains spread of the cult. It is probable that most contemporary students would agree with Shonle and Barber that prior culture did have some significance in the diffusion, both positively and negatively, rather than with Petrullo. Barber also asks the interesting question, "Do the leaders of the new cult come from among the old elite?" The answer would perhaps be negative for Taos and other fringe areas, but affirmative for the Plains, again indicating the significance of prior culture in the differential spread. Probably still more work remains to be done on the problem of differential diffusion, with respect both to individuals and to tribes, and, since peyotism is a contemporary phenomenon, perhaps such studies might serve to clarify some general problems of diffusion.[4]

Dittman and Moore have studied disturbance in dreams as related to peyotism among the Navaho, believing that "a resort to peyotism might be connected with a breakdown of traditional methods of problem solving" (1957: 643) and concluding that peyotists have more "bad dreams" according to their indices than do the non-peyotists, and that the peyotists are disapproved by the majority of the people. Newcomb, writing on Cherokee-Delaware "Pan-Indianism," says (1955: 1044) that:

> As the old culture declined and Delaware society disintegrated the void was partly bridged with a *mélange* of traits which are Indian. The peyote cult was perhaps the first, and is still one of the strongest elements furthering and cementing the bonds of Cherokee-Delaware Pan-Indianism.

Newcomb cites Devereux (1951) regarding Devereux's thesis that the common denominator of a real culture in various tribes constitutes a refractory remnant after tribal deculturation and, as such, aids the mutual reinforcement of Pan-Indianism in the separate tribes. The Pan-Indian character of peyotism has been stressed by both Slotkin and La Barre as well as other students of the cult, and would thoroughly support the position of Newcomb and Devereux. Jones's (1957) emphasis on the fact that among the Ute it was specifically the full-bloods identified with the old culture who were the peyotists, who were anti-White, and who resisted the agents' acculturative attempts, would also seem to support this contention.[5] In the Plains, however, peyotism is largely accommodative, in contrast to the Ghost Dance; and in some tribes, as in Menomini, the peyotists are at best a transitionally-acculturated group. Arth (1956) suggests an interesting refinement and thinks that the function of peyotism (and hence the differential facilitation of its spread) may be different for different age, sex, and other groups: the elderly may be concerned with health and peyote's curing function, the conservative may value its vision-producing power; the confused and half-acculturated older people may find in it a focus of resistance to the Whites; for some it may be social and recreational; and for others it may be connected with the breakdown of the Omaha male role. Newcomb (1956) has elsewhere written on the differential acculturation of the Delaware; Voget (1957) has criticized some of his conclusions in a review.

SCHOLARLY CONTROVERSIES OVER INTERPRETATIONS

The most vigorous controversialists among students of peyotism have been Stewart and Slotkin.[6] In an important tribal study of Ute peyotism, Stewart (1948) takes issue with the common belief that the peyote cult was basically aboriginal, with only secondary and adventitious accretions from Christianity. Stewart takes the view that Christian elements were early, integral, basic and essential, and diffused with the rite itself. It is possible that the specific tribe studied may give the

field worker differing opinions on this matter (though it is a little surprising perhaps that the Ute are so notably Christian). The matter may be fairly viewed only by the interested student's first-hand examination of Stewart's work. But Marvin Opler (1940) has offered an equally vigorous rebuttal to Stewart's position in a paper on the character and history of the Southern Ute peyote rite. Stewart in turn replied with a communication on the Southern Ute peyote cult (1941), and Opler again with "Fact and Fancy in Ute Peyotism" (1942). Perhaps the final solution to the problem must be left to specialists on the Ute, although historical evidence from other tribes would appear to support Opler concerning the secondary nature of Christian influences on peyotism in general.

In his 1948 monograph (p. 3), Stewart gives the interesting item that:

> Modern scientific interest in peyote was first aroused by Mrs. Anna B. Nickels, of Laredo, Texas, about 1880. From the Indians she learned of its supposedly marvelous therapeutic properties and sent samples to Parke, Davis and Co., drug manufacturers, and subsequently to scientists in Washington, D. C., Germany, France, and England for detailed and exhaustive study.

Slotkin (1955: 208, 222) disputes this and states that:

> Modern pharmacological and psychological research on peyote was begun by Briggs (1887). . . . That Briggs was the pioneer is based upon the following evidence: (a) The files of Parke, Davis & Co. on the subject of peyote begin with a clipping of his 1887 article. (b) Lewin (1888) stated that the peyote he received from Parke, Davis & Co. was obtained from Mexico. Brigg's brother lived there, and it was from him that Briggs received his own supply. (c) Lewin used the unusual form "muscale button," as did Briggs. Mrs. Anna B. Nickels is usually credited with having brought peyote to the attention of Parke, Davis & Co. I reject this for the following reasons: (a) W. P. Cusick of that company informs me that "we are unable to locate any records . . . connected with Mrs. Nickels" (personal communication). (b) Mrs. Nickels lived in Laredo, Texas. (c) She used the common form "mescal button."

Apparently her spelling, residence, and the loss of records must deprive Mrs. Nickels of the distinction of first arousing scientific interest in peyote, despite her priority in time.

Another important tribal monograph on peyote by Stewart (1944) is his *Washo-Northern Paiute Peyotism*. Stewart considers peyotism here to be purely a healing cult and cautions against "the purely sociological explanation of acculturation." His major argument in this work is a rebuttal of the cultural thesis of diffusion (1944: 94, 98):

> What is, then, the reason for diffusion? In the case of the Washo-Paiute, the individual, at times with economic motives, looms as a determining element. . . . In Ben [Lancaster], that is, the individual, rests the crucial factor in Washo-Paiute peyotism. . . . With faith shaken that cultural autopsy can adequately expose reasons for behavior and noting that in all groups there are remarkably distinct personal reactions toward introduced cults, proselytizers and their motives assume new significance. . . . Since those who decided in favor of peyote and believed it to be of great worth for curing, for salvation, and for better living had no cultural, social, or psychological status in common, but were definitely representative of all elements in the population, it is evident that each reacted as an individual, for purely personal reasons.

Since Omer Stewart is in disagreement with earlier writers on peyote in a number of particulars, his work deserves extended discussion. Stewart says that "all Peyote rituals north of the Rio Grande . . . appear universally to include elements of Christian theology and ritual integrated with aboriginal elements" although "Radin, Opler, Petrullo, La Barre, and others consider Christian elements recent additions" whereas "evidence to the contrary is presented in my Ute Peyotism" (1944: 64). La Barre, however, in a review (1946: 633), countered:

> What are these "Christian" elements anyway? *Prayers* to peyote, or via peyote to the Great Spirit? An earthen *altar*? Sage *incense*? An eagle wing-bone *whistle* (equated with the Catholic bell)? *Baptism* in the drum water? I cannot find a single demonstrably Christian element in Stewart's list of 265 traits, nor does he discuss any. Great Basin peyotism, which is recent, is not the best evidence to substantiate an argument for the near-aboriginality of Christian elements; Mexican or transitional Apache data would be more critical. Both Opler and Lumholtz are unimpressed by Christian elements in peyotism [and missionaries, from colonial times to the present, have uniformly combatted peyotism]. It is hard to see in them more than window-dressing for a proselytizing cult: considering some of the tribes' historical exposure to Christianity, it is surprising its influence is not greater.

Stewart (1944: 86) considers Shonle mistaken in her thesis that "the underlying belief in the supernatural origin of visions is important among the factors contributing to the diffusion of peyote and in a general way defines the area of its probable spread." By contrast, Stewart espouses the botanist Schultes' view that peyote's therapeutic power is more important and that "the peyote vision is incidental and of little significance" (Schultes 1938*b* in Stewart 1944: 86). But La Barre (1939) had already shown that Schultes was naïvely conceptualizing in terms of White ideology about "medicine" and that Schultes' unwitting dichotomy of "medicine power" into therapeutic pharmacodynamics and the supernatural vision created only a pseudo-problem. Plains "medicine power" is supernatural in origin (the vision), not pharmaceutical. If peyote did not cause visions, would new adherents to the cult be so ready to believe that it cures (has "medicine power")? *Of course* people take peyote partly to cure ills—*because* a vision-producing plant obviously has medicine power. La Barre further pointed out (1939: 634) that:

> For the rest, the stubborn distributional fact remains that peyotism historically has spread much as Shonle's thesis would predict: we still await in vain reports of Pueblo peyotism, except in the case of the most Plains-like, Taos. La Barre is nevertheless in error (page 90ff.) in emphasizing the "cultural compatibility" explanation of peyotism's differential diffusion. No more applicable is Kroeber's "cultural disintegration theory, which says that peoples experiencing cultural disintegration and degradation will readily accept new religions, especially those which promise the miraculous restoration of former conditions of life" (page 90). Equally unacceptable, perhaps, would be a combination of these, to the effect that peyotism was successful as a "new" Indian religion precisely because it was already compatible with the threatened aboriginal beliefs in disintegrating cultures.

When prior culture is not significant, its current disintegrated status unimportant, and Christian influences (ancient or modern) undiscussed, one wonders why indeed this is called "a study in acculturation."

Stewart seems unjust, also, in accusing earlier students of peyotism of studiously ignoring the individual. Radin's *Crashing Thunder,* after all, was first published in 1920; and La Barre wrote in *The Peyote Cult* that, "A descriptive account of a ritual pattern, however meticulously detailed it be, must always fall short of reality unless supplemented by further information regarding its functioning in individuals" (1938: 93), when introducing a chapter on the "Psychological Aspects of Peyotism" which was surely not unfriendly to the study of individual motivations. The major motivation of the proselytizer Ben Lancaster, to take money, was certainly not going to be realized if a sufficient number of cult participants were not culture-psychologically willing to give the money. Furthermore, if Ben Lancaster had motivations, do not other persons also have them? Are there not common Washo or Ute cultural assumptions basic to these motives and to the cult itself? Is not belief in the peyote cure itself cultural? The unfortunate thing is that Stewart is right so far as he goes: the individual *is* important. That Ben Lancaster was an exploiter of peyotism economically, and John Wilson was too, is interesting and significant. But to leave it at that is to perpetuate the devil theory of history-by-plot. There are also Bert Crowlance and Mary Buffalo and Jack Bear Track to be considered as well, if we are to avoid a *Führerprinzip* theory of ethnological history. The franchise of being psychologically motivated must be extended equally to the new cult's opponents and adherents too, as well as to Ben Lancaster. On this larger scale, the past nature and the current acculturative state of the culture, and various individual relationships to both, may then not seem so unimportant. The "Great Man" theory is as inadequate to explain the history of peyotism as it is of any other history, when taken alone.

Of all the students of peyotism, Slotkin was the most industrious in rediscovering colonial documents, but his supposition that they all refer to peyote has sometimes been disputed. For example, in a 1951 paper, "Early Eighteenth Century Documents on Peyotism North of the Rio Grande," he said (p. 420) with respect to an obviously unidentified tribe he specified as Comanche,

> One of these manuscripts, a report dated 1716, states that peyote was used by unspecified tribes in Texas. Another is the record of a trial held in Taos in 1720. During the proceedings it developed that an Isleta, who lived among the Hopi after the Pueblo Revolt, and now resided in Taos, had brought peyote with him from the Hopi.

This source, and this reasoning, however, are not sufficient to establish the existence of peyotism among the Comanche in 1760 nor, indeed, the cannibalism that is inferred from their *mitote*. On the contrary, if cannibalism on the part of the unspecified tribe is accepted from this tendentious colonial Spanish document, a far more plausible tribal identification would be the Tonkawa, who were known to have had peyote at an early date, and who were cannibals at least by repute. Nor do we have any reason to infer the use of peyote at Isleta Pueblo at this or any other date, at least not on the basis of these documents. Beaver also scouts Slotkin's assertion that the Hopi ever had peyote (1952: 120):

> The major idea of the article was to show the earliest mention of the use of peyote north of the Rio Grande, and to give evidence that an Isleta had brought the peyote to Taos from the Hopi. It would seem that both Slotkin and the Spanish could not distinguish peyote from jimson weed, or if it really was peyote that these Indians were using, they did not tell the truth as to its source. The Hopi were ignorant of the use of peyote and the older generations still are today.

Indeed, the phrases "the herb from Moqui" and "the herb from Aguatubi [Awatovi]" mentioned in the documents are inadequate to establish the plant involved as being specifically peyote.

> Not one ethnologist working among the Hopi has ever mentioned the use of peyote among them. Another very important factor is that the cactus does not grow in the Hopi country, nor does it grow in the country of any tribe that surrounds the Hopi. The use of the jimson weed as a medicine and by the doctors as a means of diagnosing a sickness, however, has its distribution westward to California.

The early sporadic use of peyote at Taos is also open to further research and verification; but the present documents surely do not establish the use of *Lophophora williamsii* at these dates either among the Comanche, the Isleta, or the Hopi.

Slotkin continued his researches on early documents in his paper on "Peyotism, 1521–1891" (1955). This study contains a number of valuable new references to colonial documents, but Slotkin's use of them is again open to criticism. He states (p. 202) that:

> In 1954 I had occasion to review the literature on the early history of peyotism, i.e., the use of peyote. The deeper I delved into the subject, the more unsatisfactory did the state of our knowledge appear. Consequently, it seemed useful to make a critical re-examination of the sources, so that future research might proceed on a sounder basis.

He begins his paper with a section on the "Identification of Peyote," quite as if La Barre (1938) had never devoted four appendices to the botanical identification of *Lophophora williamsii* or peyote, and the plants confused with them. Thereafter his use of documents at times only compounds confusion. For example, his inference that the Caddo (mapped as overlapping the Oklahoma-Arkansas-Louisiana-Texas border region) had peyote as early as 1709–16 is based on the bland formula that, "For purposes of this paper I attribute all 'Texas' material to the Caddo" (1955: 206). In his 1951 (p. 421) paper the unidentified tribe was Comanche! Both are wholly unwarranted, for Morris Opler's repeated studies on Texas peyotists in early times would surely have led one first to rule out such Texas tribes as the Tonkawa, Carrizo, and Lipan—and perhaps, because of the uncertain area of "Texas" in these documents, also the Coahuilteco, Jumano, and even the Tamaulipeco. Slotkin's list of the "Uses of Peyote" by various tribes is useful for its sources; but, again, this is not the first such study; and these sources should be

used critically and with caution. Slotkin concludes (1955: 208 and 210):

> The most significant result of this analysis is that the individual uses (to reduce fatigue and hunger, as a medicine, to induce "visions" for purposes of supernatural revelation, as an amulet, and as an intoxicant), and the collective use in tribal rites, all seem equally old and part of a single trait complex. Only the collective cult seems recent.

The present writer is at a loss to understand how, if the individual and collective uses in tribal rites are equally old, only the collective cult can then be recent. In the section on the "History of Peyotism," Slotkin cites peyotism for the Queres, Hopi, Isleta, Taos, Pima, Coahuilteco, and Caddo—quite as if Beaver and others had not brought some of these into grave question—and on this basis argues that "there seems to be no reason why peyotism in the north should not be as old as in the south—or at least pre-Conquest" (1955: 210). For the Coahuilteco, who live, in part, in regions where peyote grows, possibly; for the others, surely the Scottish verdict "not proven!" Perhaps it was another Texan tribe.

A minor disagreement arose over the botanist Schultes' paper on "The Appeal of Peyote (*Lophophora williamsii*) as a Medicine" (1938*b*). Schultes argued that the major reason for the spread of peyotism was its use as a "medicine" and not with reference to its vision-giving power. La Barre (1939) pointed out that Schultes' misunderstanding of the sources arose from his unawareness that "medicine" in reference to American Indians has by usage supernatural connotations, and that the *medicinal* virtues imputed to peyote were in fact based both on the visions it induces and on the "power" that the Indian thus infers is in it. The problem is purely a semantic one. Johnson (1940), incidentally, has criticized La Barre (1939) for awarding the credit for the rediscovery of *teonanacatl* to Dr. Richard E. Schultes, now Curator of the Oakes Ames Herbarium in the Harvard Botanical Museum. Johnson states that a linguist, Mr. R. J. Weitlaner of Mexico City, Johnson's father-in-law, found some mushrooms used by the Mazatec and, "recognizing the mushrooms as *teonanacatl*" of the Aztec, sent them to Dr. B. P. Reko, "who sent the specimens to botanists for identification" (1940: 549). Now, Mr. Weitlaner is certainly to be credited for his perspicacity in suspecting that the mushroom used in modern Mazatec witchcraft was probably the Aztec narcotic mushroom. But since Schultes was the first to identify the mushroom botanically as a *Basidiomycete* and to publish his results, the scientific credit would seem ultimately to be his. Perhaps this is another purely semantic problem. Can the matter be fairly stated thus: "Dr. Schultes was the first botanist to identify *teonanacatl* scientifically as a *Basidiomycete* and to publish his results"? As for Reko, he apparently misidentified *teonanacatl* with *ololiuhqui*, which is another plant, and did not himself establish the botanical identity of either.

Another point has arisen with respect to appropriate recommended usage in discussing drugs. Barber (1959) agrees with La Barre, Slotkin, and others, that peyote is not, technically, a "narcotic" since it is neither soporific nor addictive; but to remove it from the context of American Indian drug use, he thinks, is to lose the advantage of such ethnographic association. His point is well taken. Perhaps we might suggest the general term "psychotropic" for such specialized ethnographic use. In this connection it may be well to note the "mescalinismo" recently described for northern Peru by Gutérriez-Noriega (1950). The cortex of "Opuntia cylindrica 'San Pedro'" contains mescaline, for which reason the cactus has come to be used by medicine men; there is no public group ritual involved.

PEYOTE MUSIC AND ART

The major work on *Peyote Music* is by McAllester (1949). It is an excellent technical work and authoritative in its field. McAllester has also published on Menomini peyote music (1952). For technical reasons, music is an excellent way of tracing provenience and tribal influences. For example, "when peyote music [heard by McAllester in a ceremony near Window Rock] is sung by the Navaho it is rendered in the Ute musical style rather than the Navaho musical style" (Moore 1956: 220). The soundness of this method is demonstrated in the fact that the spread of peyotism from the Ute to the Navaho is thoroughly well documented for historic times by contemporary specialists on the Ute and Navaho, Stewart and Aberle, respectively. Kurath, in a review (1953: 113) of Concha Michel's *Cantos Indígenas de México* of 1951; has remarked that, "The most interesting are probably the Tarahumara sections, notably the *Canto del Peyote*, which the reviewer has found among the Navaho and the Cheyenne." Musicological evidence is thus an important adjunct to other ethnological and historical data in tracing the origins of peyotism; at the same time, the remarkable fact that the recognizably same song is found among the Tarahumara, Navaho, and Cheyenne is supported by the similar fact that in the peyote ritual meal, from ancient Mexico to modern Manitoba, the foods are always some form of boneless meat, fruit, and sweetened corn—a remarkable culture-continuity in both cases (see p. 49). Nettl (1958), in a valuable paper, has pointed out that McAllester's musicological evidence indicates the Peyote style came from the Apache to the Plains. The conclusions both of Nettl and of McAllester support the generally accepted theory of peyote's diffusion; but Nettl has also introduced an interesting principle of the differential survival of the specialized and the unspecialized that might well find application and testing in other fields of ethnography (1958: 523):

> It [the Peyote style] retained a feature of Apache music, the use of restricted rhythmic values (only two note-lengths are usually found), but in the Plains it evidently acquired the cascadingly descending, terrace-shaped melodic contour. Possibly the forces described above operated here; the melodic contour of the Plains, a specialized and rather highly developed type, was strong enough to encroach on the Peyote style, but the more generalized rhythmic structure of the Plains was not strong enough to alter the specialized rhythmic organization derived from the Apache.

Nettl (1953) has also published some interesting observations on meaningless peyote song texts which probably have linguistic and diffusionist bearing. Rhodes (1958) has published on an individual peyote song, a kind of study that is rarely done.

Without doubt the most indefatigable collector of Indian songs was the late Frances Densmore. Some of her studies have relevance to peyotism. In 1938 she wrote a paper concerning the influence of hymns on the form of Indian songs. Paired phrases, characteristic of Protestant hymns, she found in thirteen (plus six in modified form) of twenty-two Wisconsin Winnebago songs that she transcribed; paired phrases were not characteristic of the 340 Chippewa songs that have been analyzed. Densmore believed that the difference is owing to the influence of the simple flowing melodies of the Catholic Church on the Chippewa, versus the Protestant missionary influence on the Winnebago. Densmore's musicological evidence is thus consistent with the belief of Mooney, Radin, and other ethnologists that the Winnebago cult represents a more Christianized form of the peyote religion. In another paper, Densmore (1941) demonstrated on musicological grounds the syncretism of Christianity with peyotism in the Winnebago Native American Church. The marked, but atypical, position of the Winnebago and the tribes influenced by them with regard to the Christian elements in their rite must now be accepted as established by her additional evidence; but Miss Densmore was surely mistaken in her historical perspective when she argued (1941: 80), on Winnebago grounds, that:

> The peyote cult came to the Plains from other tribes and did not bring with it a ceremonial ritual. This was developed chiefly by adaptations of the customs of Christian worship.

This unfortunate error is another example of the danger of asserting propositions about peyotism at large on the basis of information from a single tribe.

The most recent work on peyote music is that by Merriam and D'Azevedo (1957) on Washo peyote songs. Some of their data on musical instruments, e.g., on the peyote rattle, are new; the description of vocal style is interesting; and their recording of songs and technical analysis of them are both competent and useful.

As is well known to ethnographers of the region, a number of American Indians in Oklahoma and the Southwest have in recent times become competent artists, quite commonly in *gouache* paintings (La Farge 1957). Some of those in Oklahoma, such as Ernest Spybuck, Stephen Mopope, and Monroe Huntinghorse (Tsa Toke), have been directly inspired in their paintings by peyotism. Recently a superb collection of Tsa Toke's paintings has been published by Denman (1957) in a limited edition by the Grabhorn Press. Monroe Huntinghorse (1904–37) was born near Saddle Mountain, Oklahoma, his grandmother being a captive White woman. He went to school at the College of Bacone at Muskogee. Huntinghorse's explanations of his pictures contain an explicit bird symbolism: the cormorant is the Water Bird of the morning ritual; the male and female yellowhammer or flicker is the Fire Bird; a parrot or a macaw, the Dawn Bird; the scissorstail or swallow, the peyote singers; while the eagle is representative of purity and love. Other information in this somewhat inaccessible and expensive ($32.00) book is worth recording here: Ida Lone Wolf, widow of the Kiowa warrior De Los Lone Wolf, said that sometimes in the old days, toward morning, if a man had great reverence, he might dance in a peyote meeting. Is this a dim echo of dancing in the peyote rituals of early Texas and colonial Mexico?

White artists have also been inspired by peyote. In Gallery One in Soho, London, in the Autumn of 1957, twenty-two fine-nibbed pen drawings were shown by the French artist, Henri Michaux, portraying visions and made after taking mescaline (*Observer* 1957). In early 1953, a German photographer named Leif Geiges sent to America a number of *Surréaliste* composite photographs that simulate mescaline intoxication, six of which were published in an American weekly magazine (*Newsweek* 1953).

POPULAR ACCOUNTS OF PEYOTE AND PEYOTISM

Several recognized ethnographers have published popular accounts relating to peyote. One of the most charming of these is a recounting by Alice Marriott in the *New Yorker* (1954) of her peyote experiences on a field trip in South Dakota. Hoebel (1949) also wrote of "The Wonderful Herb" in a literary and humanities review, describing an Indian cult vision experience among the Cheyenne of Montana. Stewart's "Three Gods for Joe" (1956) is an amusing story of a Northern Paiute who is at once a *pohari* (shaman), a peyotist, and an Episcopalian. In the same periodical is a description of "The Peyote Way" by Slotkin (1956*b*). Howard wrote on peyote in the Pan-Indian culture of Oklahoma, in an article intended for the general reader (1955). Kamffer's article on *Plumed Arrows of the Huicholes of Western Mexico* (1957) is a pleasant well-illustrated piece, including data on Huichol peyotism. Bromberg's "Storm over Peyote" (1942) was based chiefly on La Barre, and was later summarized by Schultes (1937).

Among non-anthropologists who have recently written on peyote, the most prominent and publicized undoubtedly has been the British novelist and literary man, Mr. Aldous Huxley. In recent years Huxley has become a mystic and has read widely on oriental religions and on occidental psychic research. His book, *The Doors of Perception*, appeared in 1954 and reported his experiences with mescaline. Apart from its irritating habit of inaccuracy ("[mescaline] is less toxic than any other substance in the pharmacologist's repertory" and "Professor J. S. Slotkin, one of the very few white men ever to have participated in the rites of a Peyotist congregation"), the work is highly articulate in expressing Mr. Huxley's mystical views (p. 16):

> The other world to which mescalin admitted me was not the world of visions; it existed out there, in what I could see with my eyes open. The great change was in the realm of objective fact. What had happened to my subjective universe was relatively unimportant.

These statements are not literary hyperbole for Huxley, for he believes that in mescaline intoxication he is discovering a larger *Istigkeit*. His book was somewhat variously reviewed, but provoked a symposium in the *Saturday Review* entitled "Mescalin—An Answer to Cigarettes?" by Huxley, Slotkin (who was dubious about peyote in this context), and a physician, Dr. W. C. Cut-

ting, who was guarded in his opinion. Huxley came to Durham, North Carolina, to visit his friend, Dr. J. B. Rhine, and on the evening of October 5, 1954, gave the opening address of the Duke University Lecture Series, on the subject of "Visionary Experience, Visionary Art, and the Other World." Mescaline, Huxley believes, opens the doors to another world in the "antipodes of the mind" that is as objective as an undiscovered Australia.

One of the most widely read accounts of peyote was the article "Button, Button . . ." that appeared in *Time* magazine (1951). It is also one of the most error-ridden (there is no "dismal hangover" from peyote, the ritual breakfast is not "to help straighten them out," peyote is not the "fruit" of the "mescal cactus," peyote is not "the mescal cactus," nor, incidentally, is mescal a cactus). The opinions in this article are as tendentious as its statements are erroneous. It is unfortunate that so many persons should obtain their notions about peyote from so irresponsible a source.

Journalistic surveys, often well-informed, of local or regional peyote rites have appeared increasingly in American and Canadian newspapers. One of the most workmanlike is "Indian Drums Beat Throughout Night: Peyote Users Faithful to Their Religion" (*Omaha Evening World-Herald* 1938), in which the reporter's integrity matches his knowledgeability. Aberle☆ says that the *Phoenix Republican* has also maintained a high order of journalistic accuracy in articles on peyotism. By contrast, Olcott's brochure, *The Enchanted Hills* (1948), describes peyote so misleadingly ("shaped like a prickly pear," "this pear shaped bud . . . cut from the plant," etc.) that it is clear that she has never seen a peyote plant. Other popular accounts have been of similar level, though they appeared in widely disseminated media. A Sunday supplement article, "Sent to an Artificial Paradise by the Evil Cactus Root" (*American Weekly* 1941), was based on Petrullo's *The Diabolic Root*. Perhaps the nadir has been reached in DeJacques' piece on "America's Newest Dope Horror" (1955), in which one searches with difficulty for even one accurate statement.

FUTURE STUDIES

A FEW concluding remarks might be made concerning future studies in peyotism. Detailed tribal surveys, so well begun by Kroeber and so ably continued by Aberle and Stewart and by the Spindlers, still need to be made, in particular with respect to historical diffusion, merely blocked out in large by La Barre. Both the Navaho-Ute and the Menomini studies present valuable innovations in methodology with considerable applicability to other modern field problems, and remind us that a contemporary complex such as peyotism is highly available and useful for the testing of hypotheses concerning method, diffusion, psychological and cultural issues, and the like. Problems of Pan-Indianism and acculturation will certainly continue to have relevance to peyotism in the future. Detailed psychological studies of individual and tribal peyote "theology" and related ideologies (perhaps in conjunction with the Thematic Apperception Test and other projective techniques) can still uncover for us many matters of cultural importance. Musicologists, archaeologists, and linguists are greatly needed to help solve several knotty problems. For example, we still do not know, specifically, in what area the form of the ritual first took shape, and under the influence of what tribal cultural contexts, though most experts are now agreed that peyotism first arose in the general area of the botanical provenience of peyote. Questions of differential diffusion, in particular in the Southwest, remain to be studied, perhaps in terms of the formulations of Shonle and Benedict. A putative "Red Bean Cult" as a forerunner of peyotism, early suggested by La Barre and recently reintroduced by Howard, awaits future research and definition ethnographically and historically; however, a better substitute for the unfortunate term "mescalism" could probably be found, since "mescal" (once quite erroneously confused with a narcotic mushroom) now refers confusingly to a bean, a cactus, and a succulent xerophyte, as well as to distilled pulque. And, finally, pharmacological research, now abundant on mescaline, might well be continued on the eight other alkaloids present in panpeyotl.[7, 8]

Notes

1. This is true of both Protestant and Catholic missionaries, but Aberle☆ points out that, among the Navaho, Catholic missionaries have been "particularly lenient" toward the controversial peyotism of the Navaho.

2. DAVID F. ABERLE:☆ This source "has some very sage comments on the notion of 'model' psychoses, essentially taking the position that (*a*) the similarity of the conditions produced by mescaline, LSD [lysergic acid di-ethyl amide], etc., to the functional psychoses is doubtful, but (*b*) the gross changes of affect, sensorium, motor activity, and so on created by mescaline and other substances remain very important subjects for experimentation in understanding the functioning of the nervous system, psychological processes, etc. He [Wikler] also attempts a rather thorough coverage of this type of work."

3. LOUISE SPINDLER:☆ "It might be useful to offer a few additional notes on Menomini peyotism and raise some questions on the basis of them. These notes pertain to the role of women members of the cult, which does not receive much explicit attention in the literature (but see L. Spindler 1956).

"(*a*) Relatively few female members attend meetings regularly and have vision experiences (three, in contrast to the 13 males). The Menomini peyote cult was originally for males only, and at present all important positions except one are held by males. Is it generally true that the cult is so male-dominated?

"(*b*) Many marginal female peyote-cult members use peyote in a very secular and rational manner—e.g., as medicine for earache, childbirth, etc.—with no reference to, or understanding of, its religious meaning. Others use peyote for inducing visions that furnish designs for beadwork or embroidery. These women lack identification with the cult as members, and have little involvement with the cult as a set of values and patterns for behavior. Do women peyotists elsewhere tend to be so practical and non-sacred in their point of view?

"(*c*) The female peyote-cult members comprise the only group, in the Menomini study, which is statistically differentiated from all other acculturative groups of women in specific Rorschach indices. Is there evidence that female peyotists elsewhere are psychologically

distinct in any way from non-peyotists in the same tribal community?

"(*d*) The Menomini women peyotists show striking differences from the men. The men represent an introspective, almost schizoid personality type. On the other hand, the women are outward-oriented, and not very introspective. Furthermore, their Rorschach responses suggest that they have greater control over their emotions than do the peyote men, and that they are less tense. Are male and female peyotists psychologically different elsewhere?"

4. ÅKE HULTKRANTZ:☆ "It is surprising that Miss Shonle's viewpoint on the spread of peyotism, though shared by most anthropologists, has not given impetus to further investigations. I think her theory is brilliant, and that the integration of the peyote cult with the Plains vision complex can be explained by three main conditions: (1) the old-fashioned visions—especially in correlation with dreams—could not be experienced in a society where religious doubts had resulted from the clash with White civilization, and where the natural environment, the setting for the vision quest, was encroached upon by the Whites (precisely this latter point was made by my Shoshoni informants!); (2) the peyote visions are stronger, more compelling, to the skepticist than are Plains visions (note, by the way, La Barre's quotation from Huxley, whose experiences vividly show that visions at peyote meetings are, or can be, truly hallucinatory, and are no pseudo-hallucinations like earlier visionary dreams); (3) peyote visions may be experienced by almost anybody, whereas in the old days a high percentage of Indians sought visions without success."

5. MARVIN K. OPLER:☆ "I agree with Jones's (1957) stress on cultural revivalistic elements of peyotism, which I also stressed in my Southern Ute study of 1940 and 1942. In the volume edited by Ralph Linton, *Acculturation in Seven American Indian Tribes* [New York: Appleton-Century-Crofts, 1940], I emphasized this aspect also in my chapter on the Southern Ute. This brings me to the commentaries on Omer Stewart's interpretation of the Christian elements in peyotism as being early, essential, and basic. There is a difference in the two cults on the two sides of the Southern Ute reservation, as I reported both in the *American Anthropologist* [1940*a*] and in the Linton volume in 1940 and 1942. I think it is clearly the case that Stewart has not only been, as La Barre points out, a chief controversialist representing a minority position, but that the position has added up to a curious kind of ethnocentrism in interpreting peyotism in a Christian fashion. Anthropologists are often alarmed when they find non-anthropologists guilty of this practice. It is possible that as peyotism continues in the American scene under conditions of further acculturation it will become, as it appears to have in certain places, a more Christianized cult. But these are comments only upon the pace of acculturation. The early Southwestern and Basin instances seem to have been culturally revivalistic according to practically all other scholars. This is even the case where the cult was rather abortive, as in certain Ute settings. However, this does not preclude later studies discovering a stronger element from Christian settings. My own studies appeared before Stewart's and referred to field work primarily in 1936 and 1937. Any later studies, for instance today, might have to reckon with an increased pace of acculturation."

6. ÅKE HULTKRANTZ:☆ "The discussion surrounding peyote would probably have faded away were it not for the controversies engendered by Stewart and Slotkin. La Barre has ably characterized those controversies, and exhibited a no way unfair reaction to Slotkin's papers, which in a way usurp on his own contributions. Slotkin's monograph, being basically a manual for peyotists, is not entirely on the same level with La Barre's book; still, La Barre does not stress the somewhat dogmatic character of Slotkin's opus, but justly criticizes its shortcomings in the reconstruction of paths of diffusion, and praises it as a source of legal documents and of administrative reports on the Native American Church. Many of the controversies do seem referrable to the fact that, as La Barre points out, anthropologists have asserted general propositions on the basis of data from single tribes. Further, several earlier students of peyotism never witnessed a peyote meeting, partly because the more conservative tribes resisted their presence at meetings."

7. ÅKE HULTKRANTZ:☆ "Looking to the future, I think there are two important tasks for ethnologically trained students of peyotism. First, the relations between the Mescal Bean Cult and peyotism should be further investigated. Second, research should now concentrate on the character of the peyote belief-systems, which has been very much neglected by American anthropologists, and not only in this connection; see, for example, the (in themselves excellent) studies on the Plains Indian Sun Dance, wherein beliefs have, with few exceptions, been scarcely touched on. That religious beliefs play an active role in the diffusion of peyotism has been convincingly shown by Merriam and D'Azevedo in their paper on Washo peyote songs (1957). Perhaps I should add that my interest in the *Glaubensinhalt* of peyotism is an interest also in this phenomenon per se: after all, I am looking at peyotism with European eyes."

8. GEORGE SPINDLER:☆ "La Barre's paper stimulates me to raise some questions for future research.

"First, how deep are the psychological consequences of participation in the peyote cult? Among the Menomini, the peyote complex goes very deep in the emotional, cognitive, and perceptual structuring of individual members. In ritual and its symbolism, in the supporting ideology, in conversion experiences, in art work by peyotists, and in Rorschach-test responses, there is impressive continuity and consistency. To the extent that such consistency is characteristic of other peyote groups, future research cannot ignore the psychological, as well as the social and cultural, significance of peyotism.

"Second, how widespread are the elements of peyote, or of any, ideology? Can peyotism diffuse, and be accepted, without supportive ideology? How much is the ideology reworked to fit a tribal culture, and the unique demands of each contemporary tribal community? Is the ideology reworked without major changes in ritual symbolism? The literature contains some suggestive materials relevant to these questions, but we need much more, done more systematically.

"The third problem has to do with particular combinations of events and forces operating to hinder or enhance acceptance and growth of peyotism in specific cases. La Barre mentions several of the explanations so far advanced for the diffusion of peyote, but some are essentially uni-causal in type, and others take only limited account of what seem to be essential factors. In the Menomini situation, uni-causal explanations are totally inadequate, since Menomini peyotism started with an historical accident, got under way because a few individuals were in a ready state, and finally became firmly established not from any single cause but from a combination of factors. One of these factors, for example, was a contemporary change in traditional culture which relates closely to psychology: persons possessing powers to combat witchcraft were dying out, such that other persons who were in fear of sorcery turned instead to the peyote cult for protection.

"In short, future research on peyotism must operate in several dimensions —the historical, cultural, ideological, psychological, and social—if it, or any so-called 'nativistic movement,' is to be understood."

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A Bibliography of Anthropological Bibliographies: The Americas

Compiled under the direction of *Gordon D. Gibson*

THIS bibliography of bibliographies on the Americas constitutes an in-process compilation of the most significant anthropological bibliographies originally published before 1955. The several serial compilations of recent titles in anthropology, announced in 1958, should, at long last, provide continuing and exhaustive guides to the very rapidly expanding current anthropological literature. One of these, the *International Bibliography of Social and Cultural Anthropology* (Paris: UNESCO), first issued for general distribution in 1958, covers items published since 1954.* The others, the *Quarterly Check-list of Ethnology and Sociology, Quarterly Check-list of Linguistics, Quarterly Check-list of Oriental Art and Archeology, Quarterly Check-list of Oriental Religions,* and the *Quarterly Check-list of Oriental Studies,* published by the American Bibliographic Service (East Northport, N. Y.) have begun to provide additional coverage in certain fields wholly or partly within anthropology.

It seems reasonable to assert that no bibliography is ever complete; the principle of diminishing returns on the time expended in searching for additional items provides a practical limitation, and, for practical purposes also, certain more or less arbitrary boundaries must be established. This bibliography of bibliographies is limited as follows:

a) Lists of anthropological references which do not pertain primarily to America have been excluded. In accord with this decision, general anthropological bibliographies, including periodical compilations of current literature—for example, those appearing in the *American Anthropologist* from 1915 to 1949—are omitted, even when they are organized in such a way as to bring the American materials together in specific sections. But periodical compilations which are devoted chiefly or exclusively to anthropological literature concerning America have been included if they continue for two or more years.

b) Compilations of the titles of works of particular anthropologists have been withheld.

c) Lists of references cited in books and articles and appended to them have, save in exceptional cases, been excluded. When such lists are particularly comprehensive, they properly deserve the name "bibliography" and ought to be included in this compilation. Those of this variety known to the compiler or brought to his attention by his colleagues have been included, but doubtless numerous other excellent bibliographies appended to books or articles have been omitted.

d) Bibliographies of works in fields that are related to but generally considered distinct from anthropology have, as a rule, been excluded, except when they contain significant sections which properly fall within the field of anthropology. The researcher is advised to consult the available and often excellent bibliographies in regional geography, history, literature, art, biology, medicine, geology, economics, sociology, political science, etc., when dealing with a subject that lies in part in an adjacent field. Such well-known historical sources as Harrisse's *Bibliotheca Americana vetustissima* and Sabin's *Bibliotheca Americana* have been excluded because of their marginality.

e) Cumulated indexes to periodicals are included only when they are published as articles. The separately published indexes to many periodicals are listed in the *Union List of Serials.*

f) Book-review series are not listed, even when the reviews are numerous. Lists of books received by a journal for review also are not considered bibliography for the purposes of this compilation.

g) Unbound bibliographies reproduced by office machines and given limited distribution are, in general, omitted. Exceptions have been made in a few cases where the subject is not covered by any other known published bibliography, and where copies have been deposited in public libraries. Such bibliographies are here termed "processed."

h) Compilations of the titles of the anthropological publications of particular institutions are included only when they are in some degree analytical.

i) Catalogs of particular library collections are not included unless they contain numerous items not elsewhere listed.

j) Lists of unpublished documents pertaining to America

* This was preceded by a mimeographed compilation in provisional form: *International Bibliography of Socio-Cultural Anthropology* (Paris? 1955), with introduction by Georges Balandier; chief compiler, M. L. Bernot. It covers publications which appeared in 1954.

GORDON D. GIBSON is Associate Curator of Ethnology at the United States National Museum, Smithsonian Institution, Washington, D.C. He was born in 1915, and educated at the University of Chicago. After receiving his Ph.D. in 1952, he did a year of field work among the Herero in Bechuanaland Protectorate, has published "Double Descent and Its Correlates among the Herero of Ngamiland" (*American Anthropologist* 58: 109–39), and has a report on the mating system of the Herero in press.

Before joining the Smithsonian staff in 1958, Gibson served as Assistant Professor of Anthropology at the University of Utah. He began the compilation of this bibliography while there, because he believed that "it would be a useful aid for students, would help refute the not uncommon allegation that anthropological knowledge is esoteric, and would provide the basis for an eventual definitive revision of great value to the profession."

and guides to manuscript collections have, in most cases, been omitted. The following guides are recommended to students interested in searching archival files:

> Billington, Ray A. "Guides to American history manuscript collections in libraries of the United States," *The Mississippi Valley Historical Review*, **38**, *3*, 1951: 467–96.
>
> *Archivum; Revue Internationale des Archives*. Paris: UNESCO, **1**+, 1951+.

k) As indicated above, bibliographies first published in 1955 or later are, with few exceptions, omitted. However, recent revisions of older bibliographies are included.

This bibliography is arranged by geographical areas, and is followed by a combined author and subject index. The geographical divisions employed conform more to the distribution of the bibliographies than to any particular culture-area scheme. The entries have been classified according to the broadest geographical category into which they might fall, and generally are not cross-indexed in the sub-categories. Therefore, for example, when looking for bibliographies dealing with Western U.S.A., one must look also under the broader categories: America, N. America, and U.S.A.

In the abbreviated form of collation statement adopted for the entries, volume is shown in bold face, number in italics (whether issues are numbered within successive, or consecutively through all, volumes), and year in light-face type. A colon precedes the page reference.

In order to indicate one library within the United States at which each bibliography may be consulted or from which a copy may be secured, a library designation and call number are provided immediately following the collation statement. In most cases, the Library of Congress (DLC) is indicated and its call number provided. In those cases in which the Library of Congress lacked a copy at the time of this compilation, the library in which a copy is known to be deposited is indicated by the corresponding National Union Catalog symbol. The library symbols employed in this compilation and the corresponding institutions are the following:

CU*	University of California, Berkeley 4, California
DGS	U.S. Geological Survey, Washington 25, D.C.
DI	U.S. Department of the Interior, Washington 25, D.C.
DJ	U.S. Department of Justice, Washington 25, D.C.
DLC*	Library of Congress, Washington 25, D.C.
DPU*	Pan-American Union, Washington 6, D.C.
DSI*	Smithsonian Institution, Washington 25, D.C.
ICN*	Newberry Library, Chicago, Illinois
ICU*	University of Chicago, Chicago 37, Illinois
NcD*	Duke University, Durham, North Carolina
OU*	Ohio State University, Columbus 10, Ohio
PPAN	Academy of Natural Sciences, Philadelphia, Pennsylvania
UU	University of Utah, Salt Lake City 1, Utah.

Those of the above libraries which are prepared to provide microfilm copies of the items in their possession are indicated by asterisks after the library symbol. Most of the above libraries regularly lend books to other responsible libraries within the United States.

The annotation proper follows the collation statement. In the abbreviated formula herein adopted, the number at the beginning of the annotation refers to the number of entries in the bibliography, approximations being indicated by the symbol ±. Then follows information on, respectively, the hierarchy of classification employed in the bibliography, the kind of indexes provided, and the kind of annotation. No mention of either a scheme of classification, an index, or an annotation signifies that the bibliography lacks the unmentioned feature. Any special remarks appear at the end.

The subject entries in the index are in the main a guide to content as indicated in the titles of the bibliographies. The more general works also should, of course, be consulted in pursuing research on particular topics.

I should like here to acknowledge my indebtedness to the compilers of earlier bibliographies upon whose works I have drawn freely. I wish also to acknowledge the many helpful suggestions received from my colleagues, and the valued assistance of Mrs. Carol C. Stout and Mrs. Bethune M. Gibson in the examination of the items listed and in the preparation of the entries. Special recognition is due Miss Marian L. Sheets, Head Cataloguer of the University of Utah Library, for advice often sought and freely given. The research was partly supported by a grant from the University of Utah Research Fund.

The compiler will appreciate notice of important

ABBREVIATIONS†

B.A.E. Bureau of American Ethnology, Smithsonian Institution, Washington, D. C.
B.B.A.A. *Boletín Bibliográfico de Antropología Americana*, Mexico: Instituto Panamericano de Geografía e Historia. 1+ 1937+
bib. bibliography
chron. chronological
comp. compiler
ca. about
Dept. Department
geog. geography, geographical
ed., eds. editor(s), edition(s)
H.L.A.S. *Handbook of Latin American Studies* 1935+. Gainesville, Florida: Univ. of Florida Press, 1936+. Annual. (Pub. in Cambridge, Mass., by Harvard Univ. Press, 1936–47.)
illus. illustrated, illustrations
Jour. Journal
M.A.I., H.F. Museum of the American Indian, Heye Foundation, New York
n.d. no date
n.s. new series
no. number(s)
p. page(s)
pl. plate(s)
pt. part(s)
q. v. *quod vide* (which see)
rev. revised
Soc. Society
suppl. supplement
Univ. University
U. S. United States of America
vol. volume(s)

† Also abbreviated are names of states of the U.S., and months of the year.

1. America, General

BERCAW, LOUISE O., ANNIE M. HANNAY, and NELLIE G. LARSON. *Corn in the development of the civilization of the Americas; a selected and annotated bibliography.* (U. S. Bureau of Agricultural Economics, Agricultural Economics Bibliography, *87.*) Washington, D.C.: 1940. 195 p. Processed. DLC—Z5074.E3U35 no. 87

405. Author. Index: author and subject. Extensive annotation. [1

"Bibliografía antropológica americana," *B. B. A. A.*, 1–4, 1937–40. Annual. DLC—Z5112.P3

250–750 ±. Periodical literature and books, author. Some items annotated. Compiled by Rafael H. Valle. Early issues pertain to Latin America only. Publication of current bibliography in the *B. B. A. A.* was resumed in 1949 as "Recent publications." [2

"Bibliographie américaniste," *Journal de la Société des Américanistes de Paris*, n. s. 11+, 1914–19+ Annual. DLC—E51. S68

1,000–2,000 ±. Subject, area, author. Compilers: P. Rivet, P. Barret, S. Lussagnet, M. Doré, and others. [3

"Bibliography of the American Ethnological Society 1842–1942," *American Anthropologist*, **45**, *2*, 1943: 241–43. DLC—GN1.A5 vol. 45

58. Author. Compiled with the assistance of Daniel Spencer. [4

BOEHMER, GEORGE H. "Index to papers on anthropology published by the Smithsonian Institution, 1847 to 1878," *Smithsonian Institution Annual Report, 1879* (Washington, D.C.: Government Printing Office, 1880) : 476–83. DLC—Q11.S66 1879

210 ±. Author, locality, and subject (interlisted). [5

BONNERJEA, BIREN. "General index, annual reports of the Bureau of American Ethnology, vols. 1 to 48," *U. S. Bureau of American Ethnology, Forty-eighth Annual Report, 1930–1931* (Washington, D.C.: Government Printing Office, 1933): 25–1220. DLC—E51.U55 vol. 48

154 scientific papers. Subject index, chronological list of the papers by volume, index to authors and titles. [6

BRINTON, DANIEL G. *Aboriginal American authors and their productions.* Philadelphia: 1883. 63 p. Bib.: *passim.* DLC—PM108.B8

100 ±. Indexes: author, tribe. [7

[BUTLER, RUTH LAPHAM.] *A bibliographical check list of North and Middle American Indian linguistics in the Edward E. Ayer collection.* Chicago: Newberry Library, 1941. 2 vol., 509, 417 p. DLC—Z7116.N4

5,000 ±. Language, author. Index: author, editor, translator. [8

CASTRO, EUGENIO DE. "Relação bibliográfica de linguística americana," *Ameríndia* (Rio de Janeiro: Serviço Gráfico do Ministerio de Educação e Saúde, 1ª serie, **1**, *1*, 1937. 129 p. DLC—Z7116.R58 no. 1

1,000 ±. Title. Many of the items are chapters of books, listed by chapter title. [9

"A catalogue of works on the aboriginal languages of America," *Bibliotheca hispano-americana* (London: Trübner, 1870): 162–84. DLC—Z1610.T86

130 ±. General and tribe or language, author. Annotated. [10

COMAS, JUAN. "Aportación a la bibliografía y estadística serológica racial americana," *B. B. A. A.* **5**, 1941: 29–37. DLC—Z5112.P3 vol. 5

100. Author. Table following bibliography compiles serological data. [11

COMAS, JUAN. *Bibliografía selectiva de las culturas indígenas de América.* (Instituto Panamericano de Geografía e Historia, Publicación, *166;* Comisión de Historia, Publicación, *61*, Bibliografías, *1.*) Mexico: 1953. 284 p., maps. DLC—F1401. P153

2,014. Time period, subject, area, author. Indexes: tribe, author. Brief annotation. [12

COMAS, JUAN. "Estudios somáticos sobre el negro americano, publicados en el American Journal of Physical Anthropology: recopilación y sistematización," *B. B. A. A.*, **10**, 1947: 121–32. DLC—Z5112.P3 vol. 10

170. Subject, author. Index: author [13

COMAS, JUAN. *Los congresos internacionales de americanistas. Síntesis histórica e índice bibliográfico general, 1875–1952.* Mexico: Instituto Indigenista Interamericano, 1954. 224 p. DLC—E51.I73

2,127. Subject, general and region, author. Index: author. Sparsely annotated. [14

DOCKSTADER, FREDERICK J. *The American Indian in graduate studies: a bibliography of theses and dissertations.* (M. A. I., H. F., Contributions, **15**.) New York: 1957. 399 p. DLC—E51.N42 vol. 15

3,684. Author. Index: subject. Brief annotation, including year, institution to which submitted, and degree earned. Includes "all graduate studies which consider the Indian to the extent of at least one chapter—either by specific section, or scattered throughout *in passim.*" Includes archaeology. Indexes theses from 203 schools. [15

DOUGLAS, FREDERIC H. *A guide to articles on the American Indians in serial publications, part 1.* Denver: Art Museum, Department of Indian Art, 1934. 332 p. Processed. DLC—Z1209.D35

6,300 ±. Serial, date, title of article. Lists articles in 119 serials issued by 80 organizations. Covers chiefly the American, Canadian, and English journals. Later parts not published to date. [16

EDWARDS, EVERETT E., and WAYNE D. RASMUSSEN. *A bibliography on the agriculture of the American Indians.* (U. S. Dept. of Agriculture, Miscellaneous Publications, *447.*) Washington, D.C.: Government Printing Office, 1942. 107 p. DLC—Z1209.E27

841. Subject. Index: author, subject, tribe, etc. Annotated. [17

"Folklore bibliography," *Southern Folklore Quarterly*, 2+, 1938+ DLC—GR1.S65

150–500 ±. Subject, area, author. Annotated. The compilation in **2**, 1938, pertains to North America only, later issues include Latin America. Compiled by Ralph S. Boggs. [18

"Folklore in periodical literature," *Journal of American Folklore*, **62**, *244*–**66**, *262;* 1949–53. Quarterly. DLC—GR1.J8

Early issues list by author and title the contents of current North American serial publications in folklore and scattered articles in other journals. Later issues are world-wide in scope. Compiled by R. M. Dorson, S. A. Gallacher, and W. E. Richmond. Continued in the "Annual folklore bibliography" in the same journal. [19

FRIEDERICI, GEORG. *Amerikanistisches Wörterbuch.* (Univ. von Hamburg, Abhandlung aus dem Gebiet der Auslandskund, **53**: Reihe B, Völkerkunde, Kulturgeschichte und Sprachen, **29**.) Hamburg: 1947. 722 p. "Verzeichnis der benutzten, mehr als einmal angeführten Schriften": 679–704. DLC—PC4822.F7

1,000 ± items cited more than once, plus an indefinite number cited once only. A historical dictionary of Americanisms (mostly words from American Indian languages) in German, Spanish, French, English, Portuguese, and Dutch, with detailed references to the literature. List of synonyms appended. Supersedes his *Hilfswörterbuch für den Amerikanisten* (Halle: 1926) . [20

FRIEDERICI, GEORG. *Skalpieren und ähnliche Kriegsgebräuche in Amerika . . .* (Inaugural-Dissertation, Univ. of Leipzig, 1906.) Braunschweig: Vieweg, 1906. "Verzeichnis der benutzten quellen": 138–70. DLC—E98.W2F8

900 ±. Area, author. [21

FRIEDERICI, GEORG. "Zu den vorkolumbischen Verbindungen der Südsee-Völker mit Amerika," *Anthropos*, **24**, *3–4*, 1929: 441–87. Bib.: *passim.* DLC—GN1.A7 vol. 24

200 ±. Text arranged topically. [22

GARCÍA ICAZBALCETA, JOAQUÍN. *Apuntes para un catálogo de escritores en lenguas indígenas de América.* Mexico: 1866. 157 p. DLC—Z7116.G21

——— Reprinted in *Obras de D. J. García Icazbalceta*, tomo 8. Mexico: V. Agüeros, 1896–99. DLC—F1203.G24 vol. 6

175. Pt. 1 "Libros de mi coleccion," Pt. 2 "Libros de diversas colecciones;" author. Index: languages. Annotated. [23

HENRÍQUEZ UREÑA, PEDRO. *Para la historia de los indigenismos; papa y batata, el enigma del aje, boniato, caribe, palabras antillanas.* (Univ. de Buenos Aires, Instituto de Filología, Biblioteca de Dialectología Hispanoamericana, Anejo 3.) Bucnos Aires: 1938. 147 p. Bib.: 7–14. DLC—PC4822.H4

120 ± plus some items mentioned *passim.* Author. Indexes: author, place, language, local names of plants, botanical names. [24

HENRY, MELLINGER E. *A bibliography for the study of American folk-songs. . . .* London: Mitre Press, 1937. 142 p. DLC—ML120.U5H5. Reprinted from *The Book Trade Journal, 13*, Aug. 10, 1935, to *47*, Apr. 3, 1936. DLC—Z1007.B7168

2,500 ±. Author. Annotated. Includes songs of non-American origin. [25

HRDLIČKA, ALEŠ. *Melanesians and Australians and the peopling of America.* (Smithsonian Miscellaneous Collections, **94**, *11:* Publication, *3341.*) Washington, D.C.: Government Printing Office, 1935. 58 p. DLC—Q11.S7 vol. 94

104. "Bibliographic notes" (extracts) and "Literature cited," author. Concerns native American racial origins in general. [26

LECLERC, CHARLES. *Bibliotheca americana. Historie, géographie, voyages, archéo-*

logie et linguistiques des deux Amériques et des îles Philippines. Paris: Maisonneuve, 1878. 739 p. DLC—Z1207.L463

2,638. Subject (I–history, II–linguistics), area, topic, author. Index: author and title. Annotated.

——— Supplement, no. 1. Paris: Maisonneuve, 1881. 102 p.
391. Author. Annotated. DLC—Z1207.L463

——— Supplement, no. 1. Paris: Maisonneuve, 1887. 110 p.
591. Author. Annotated. DLC—Z1207.L463 [27

LESSER, ALEXANDER. "Bibliography of American folklore, 1915–1928." *Journal of American Folk-lore,* **41,** *159,* 1928: 2–60. DLC—GN1.J8 vol. 41

1,200 ±. Area and major ethnic group, author. Index: tribal (American Indians north of Mexico only). [28

List of publications of the Bureau of American Ethnology, with index to authors and titles. Revised to June 30, 1956. Washington, D.C.: Government Printing Office, 1956. 112 p. DLC—Z5114.U58 1956

Lists the contents of 72 Annual Reports and 167 Bulletins of the B. A. E., 16 publications of the Institute of Social Anthropology, 9 vols. of Contributions to North American Ethnology, 4 vols. of Introductions, and 13 Miscellaneous Publications of the B. A. E. Indexes: author and title. Volumes out of print noted. [29

List of publications of the Museum of the American Indian, Heye Foundation. 8th ed. (Indian Notes and Monographs [Miscellaneous], *49*), 1933; 4th printing, rev., New York: 1946. 32 p. DSI—570.733 no. 49, 1946.

Lists contents of the Contributions, Contributions to South American Archeology, Indian Notes and Monographs, and Leaflets, but not of Indian Notes. Index: author and title. [30

LUDEWIG, HERMANN E. *The literature of American aboriginal languages. With additions and corrections by* WILLIAM W. TURNER. London: Trübner, 1858. 258 p. DLC—Z7116.L94

2,500 ±. Language and region, tribe. Indexes: language and synonym. [31

MATTFIELD, JULIUS. *The folk music of the Western Hemisphere; a list of references in the New York Public Library.* New York (City): Public Library, 1925. 74 p. DLC—ML136.N5M2; Z6814.F7N5.

1,000 ±. Ethnic group, author. Index: author. Brief annotation. Appendix: primitive musical instruments in America. [32

MILLER, MAMIE R. T. *An author, title, and subject check list of Smithsonian Institution publications relating to anthropology.* (Univ. of New Mexico, Bulletin, Bibliographical Series, **1,** *2;* whole no. *405.*) Albuquerque, New Mexico: Univ. of New Mexico Press, 1946. 218 p. DLC—Z5111.M5

1,100 ±. Author. Indexes: author, title, subject. Complete to 1942. [33

MITRE, BARTOLOME. *Catálogo razonado de la sección lenguas americanas* (Museo Mitre, Buenos Aires). Buenos Aires: Hermanos, 1909. 3 vol. DLC—Z7116.B92

500 ±. Linguistics and languages or areal language groups, author. Indexes: author, language. Extensive annotation. [34

"Recent publications." *B. B. A. A.,* 12+ 1949+ Annual. DLC—Z5112.P3

250–600 ±. Subject, author. [35

"Revista de revistas," *B. B. A. A.,* 2+ 1938+ Annual. DLC—Z5112.P3

Tables of contents of many serial publications in anthropology, chiefly American. [36

RIVET, PAUL, and H. ARSANDAUX. *La métallurgie en Amérique précolombienne.* (Univ. de Paris, Institut d'Ethnologie, Travaux et Mémoires, **39.**) Paris: Musée de l'Homme, 1946. Bib.: 189–210. DLC—GN778.S6R5; GN2.P2 vol. 39

390 ±. Works consulted and not consulted, author. [37

SCHULLER, RODOLFO R. *Lingüística americana, notas bibliográficas.* Madrid: Tip. de la Revista de Archivos, Bibliotecas y Museos, 1912. 42 p. DLC—Z7122.A353

250 ±. Type of item (pamphlet, manuscript, etc.), chron. Brief annotation. [38

SELLARDS, ELIAS H. *Early man in America; a study in prehistory.* Austin, Texas: Univ. of Texas Press, 1952. Bib.: 155–205. DLC—E58.S44

900 ±. Author. Supersedes bibliographies in author's articles in *Bulletin of the Geological Society of America,* **51,** *3,* 1940; 373–431, and **58,** *10,* 1947: 955–77. [39

"Some recent publications," *American Antiquity.* Quarterly **1–6,** 1935–36 to 1940–41; triennial **7,** 1941–42; annual **8+,** 1942–43+ DLC—E51.A52

100–450 ±. Subject and region, author. [40

SWADESH, MORRIS. "Bibliography of American Indian linguistics, 1936–37," *Language,* **14,** *4,* 1938: 318–23. DLC—P1.L3 vol. 14

61. Author. Continued in compilation by Carl F. Voegelin under same title. [41

VIÑAZA, [CIPRIANO MUÑOZ Y MANZANO] (conde). *Bibliografía española de lenguas indígenas de América.* Madrid: Biblioteca Nacional, 1892. 435 p. DLC—Z7120.V794

1,241. Part I—works whose publication has been confirmed by the compiler, Part II—works in manuscript or whose publication was not confirmed; chron. Indexes: language and area, author or translator, important persons mentioned in Part I. Annotated, some items extensively, including some summaries of grammars. [42

VOEGELIN, CARL F. "Bibliography of American Indian linguistics 1938–41," *Language,* **18,** *2,* 1942: 133–39. DLC—P1.L3 vol. 18

107 (including journals). Area, general and language family. A sequel to MORRIS SWADESH, "Bibliography of American Indian linguistics 1936–37." [43

VOEGELIN, CARL F., and ZELLIG S. HARRIS. *Index to the Franz Boas collection of materials for American linguistics.* (Language Monograph, *22;* Supplement to *Language,* **22,** *3,* 1945.) Baltimore, Md.: Linguistic Society of America. 43 p. DLC—Z7116.V6

380. Manuscripts. Indexes: language, author, area. Includes some nonlinguistic materials. [44

WAGNER, G., and M. GUSINDE. "Amerika," in "Völkerkundliche Bibliographie von 1928 an, mit Nachträgen der Jahre 1924–1927," *Ethnologischer Anzeiger, 3, 1–2,* 1932–34: 1–7. CU—GN1.E75 vol. 3

130 ±. Area, author. Chiefly 1928–31, with a few earlier items. [45

1.1 North America

BEERS, HENRY P. *Bibliographies in American history; guide to materials for research.* Rev. ed. New York: Wilson, 1942. 487 p. DLC—Z1236.A1B4

11,500 ±. Author. Index: subject, including tribes. Contains sections on Indians (90 items); Negro and slavery (64 items); other races (43 items). [46

BREW, JOHN O. *A selected bibliography of American Indian archaeology east of the Rocky Mountains.* (Excavators' Club, Papers, **2,** *1.*) Cambridge: 1943. 90 p. DLC—CC21.E85 vol. 2, no. 1

1,300 ±. Subject and region, author. [47

BUTTERFIELD, LYMAN H., W. E. WASHBURN, and W. L. FENTON. "A bibliography," in W. L. FENTON, *American Indian and White relations to 1830. Needs and opportunities for study* (Chapel Hill: Univ. of North Carolina Press, 1957): 31–138. DLC—Z1209.F38

700 ±. Type of source and topic, author. Index: author, tribe, place, and subject. Annotated. [48

Catalogue of the library belonging to Mr. Thomas W. Field. New York: Bangs, Merwin & Co., 1875. 376 p.
——— Supplement. New York: Shelley, 1875. 59 p. DLC—Z1209.F456

2,664 in first catalog, 661 in supplement. Author or title. Some items annotated. Many items dealing with North American Indians. [49

CROFT, KENNETH. "A guide to source material on extinct North American Indian languages," *International Journal of American Linguistics,* **14,** *4,* 1948: 260–68. DLC—PM101.I5 vol. 14

154. Language family, language, author. Brief annotation. [50

DE PUY, HENRY F. *A bibliography of the English colonial treaties with the American Indian.* New York: Lenox Club, 1917. 109 p. DLC—Z1209.D37

50. Chron. Synopsis of each treaty. Location of copies. [51

ELLIS, GEORGE E., and JUSTIN WINSOR. "The red Indian of North America in contact with the French and English; critical essay on the sources of information," in JUSTIN WINSOR (ed.), *Narrative and critical history of America.* I. Boston: Houghton, 1884–89. Bib.: *passim,* 316–28. DLC—E18.W77 vol. 1

250 ±. Area. Index to vol. I includes references to this section. Evaluative annotation. [52

FIELD, THOMAS W. *An essay towards an Indian bibliography. Being a catalog of books relating to . . . the American Indians, in the library of Thomas W. Field.* New York: Scribner, Armstrong, and Co., 1873. 430 p. DLC—Z1209.F45
——— Reprint. Columbus, Ohio: Long's College Book Co., 1951. ICN

1,700 ±. Author. Annotated. See also *Catalogue of the library belonging to Mr. Thomas W. Field.* [53

FOSTER, GEORGE E. *Literature of the Cherokees.* Ithaca, N. Y.: Office of the Democrat, 1889. 110 p. Bib., separately paginated, 28 p. DLC—Z7119.C4F7

200 ±. Some items annotated. Includes items by or about Cherokee Indians, in English or in Cherokee. [54

GAINES, RUTH L. *Books on Indian arts north of Mexico.* New York: Exposition of Indian Tribal Arts, Inc., 1931. 15 p. DLC—Z1209.E96

650 ±. Subject (lists and indexes, arts and their interpretation, dances and ceremonies, legends and folklore, music and songs), author. [55

HARDING, ANNE D., and PATRICIA BOLLING. *Bibliography of articles and papers on North American Indian art.* Washington, D.C.: U. S. Dept. of Interior, Indian Arts and Crafts Board, n.d. [*ca.* 1937]. 365 p. Processed. DLC—Z1209.H26

1,600 ±. Author. Indexes: area and tribe, craft. Brief annotation. Comprises chiefly periodical publications of museums and universities. [56

HARGRETT, LESTER. *A bibliography of the constitutions and laws of the American Indians.* Cambridge, Mass.: Harvard Univ. Press, 1947. 124 p. DLC—Z1209.H28

250 ±. Tribe, chron. Indexes: persons, places, tribes, topics. Annotated. [57

HAYWOOD, CHARLES. *A bibliography of North American folklore and folksong.* New York: Greenberg, 1951. 1292 p. DLC—Z5984.U5H3

30,000 ±. Whites and Indians, major subject area, author. Index: author, title, place, subject. Brief annotation of some items. [58

HODGE, FREDERICK W. (ed.). *Handbook of American Indians north of Mexico.* (U. S. Bureau of American Ethnology, Bulletin, *30.*) 2 parts. Washington, D.C.: Government Printing Office, 1907–1910. Reprint, 1912. Bib.: part 2, p. 1179–1221. DLC—E51.U6 no. 30; E77.H68; E77.H69

—— Reprint of bibliography, 1912. DLC—Z1209.H64

2,450 ±. Author. This is a list of references cited in the articles in the body of the Handbook. [59

HRDLIČKA, ALEŠ. "Anthropology of the American Negro," *American Journal of Physical Anthropology,* **10**, *2*, 1927: 205–35. Bib.: 216–35. DLC—GN1.A55 vol. 10

400 ±. General and subject, author. Index: author. [60

Indian captivities and massacres; being the contemporary record of Caucasian contact and conflict with the native American; books, pamphlets, and broadsides offered for sale. New York: Eberstadt and sons, 1943. 70 p. DLC—Z1209.E23

398. Author. Index: geographical. Annotated. Reproductions of some title pages. [61

"Indians," *Bulletin of the Riverside Public Library* (Riverside, Calif.), *136*, 1916: 1–24. DLC—Z1209.R59

300 ±. Subject, author. Designed to be "representative rather than complete." [62

JONES, JOHN ALAN. *List of unpublished doctoral dissertations and masters theses in the field of anthropology bearing on North American Indians north of Mexico.* Washington, D.C.: Department of Justice [1953]. 19 p. Processed. DJ—Z5114.J6

169. Subject, author. [63

KELSAY, LAURA E. *List of cartographic records of the Bureau of Indian Affairs* (*Record group 75*). (U. S. National Archives, Publication, *55–1*; Special lists, *13.*) Washington, D.C.: 1954. DLC—CD3035.B8U5

540. I—Central Map Files, II—cartographic records of the Irrigation Division; area. [64

MURDOCK, GEORGE P. *Ethnographic bibliography of North America.* 2nd ed. New Haven: Human Relations Area Files, 1953. 239 p. DLC—Z1209.M8 1953

12,800 ±. First edition list and supplement, area, tribe, author. Appendix: general North America. Detailed table of contents. Tribal map. Includes some tribes of northern Mexico. [65

Narratives of captivity among the Indians of North America. A list of books and manuscripts on the subject in the Edward E. Ayer collection of the Newberry Library. (Newberry Library, Publication *3.*) Chicago: 1912. 120 p. DLC—Z1209.N53

—— —— Supplement I. Clara A. Smith, comp. Chicago: 1928. 49 p. DLC—Z1209.N53 suppl.

339 in original item, 143 in supplement. Author. Index: names of captives. Annotated. [66

PILLING, JAMES C. *Bibliography of the Algonquin languages.* (U. S. Bureau of Ethnology, Bulletin, *13.*) Washington, D.C.: Government Printing Office, 1891. 614 p. DLC—E51.U6 no. 13

2,500 ±. Author or translator, interlisted with cross-references by vernacular title, subject, and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. Facsimiles of title pages or first pages of some items. [67

PILLING, JAMES C. *Bibliography of the Athapascan languages.* (U. S. Bureau of Ethnology, Bulletin, *14.*) Washington, D.C.: Government Printing Office, 1892. 125 p. DLC—E51.U6 no. 14

544. Author or translator, interlisted with cross-references by subject and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. Facsimiles of selected pages. [68

PILLING, JAMES C. *Bibliography of the Siouan languages.* (U. S. Bureau of Ethnology, Bulletin, *5.*) Washington, D.C.: Government Printing Office, 1887. 87 p. DLC—E51.U6 no. 5

550 ±. Author or translator, interlisted with cross-references by vernacular title, subject, and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. [69

PILLING, JAMES C. *Bibliography of the Wakashan languages.* (U. S. Bureau of Ethnology, Bulletin, *19.*) Washington, D.C.: Government Printing Office, 1894. 70 p. DLC—E51.U6 no. 19

251. Author or translator, interlisted with cross-references by subject and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. Facsimiles of selected pages. [70

PILLING, JAMES C. *Proof-sheets of a bibliography of the languages of the North American Indians.* (U. S. Bureau of Ethnology.) Washington, D.C.: Government Printing Office, 1885. 1135 p. DLC—Z7118.P64

125 ± sources, 4,308 entries, and 1,000 ± additions. Author or translator or title. Index: language, dialect, or area. Many items annotated; location of copies indicated. Facsimiles of title pages of 29 rare items. Includes manuscripts. Only 110 copies of this "preliminary, tentative, and incomplete catalogue" were printed. Superseded in certain language families by Pilling's later publications in the Bulletins of the Bureau of Ethnology. [71

POWELL, JOHN W. "Literature relating to the classification of Indian languages" in "Indian linguistic families of America north of Mexico," *U. S. Bureau of Ethnology, 7th Annual Report, 1885–1886* (Washington, D.C.: Government Printing Office, 1891): 1–142. Bib.: 12–25. DLC—E51.U55 vol. 7

39. Chron. Extensive annotation. Concerns earlier attempts at classification of native languages of North America. [72

SCHMECKBEIER, LAURENCE F. *The Office of Indian Affairs; its history, activities and organization.* (Institute for Government Research, Service Monographs of the United States Government, *48.*) Baltimore: Johns Hopkins Press, 1927. 591 p. Bib.: 537–80. DLC—E93.S34

680 ±. Subject (general and historical, special tribes, education and citizenship, finance and trade conditions, health and social conditions, property questions, natural resources, law and legal questions, reservations, bibliography), author. Some items briefly annotated. Includes only those publications of the Indian Affairs Office which treat of the services, work, and problems of the Office. [73

SCHOOLCRAFT, HENRY R. *A bibliographical catalogue of books, translations of the Scriptures, and other publications in the Indian tongues of the United States, with brief critical notices.* Washington, D.C.: Alexander (printer), 1849. 28 p. DLC—Z7118.S37

139. Language family, tribe. Annotated. [74

"A selected bibliography on the physical and mental abilities of the American Negro," *Journal of Negro Education,* **3**, *3*, 1934: 548–64. DLC—LC2701.J6 vol. 3

563. Time periods, subject, author. Includes unpublished theses. [75

Subject index of Indian Office circulars, nos. 160–1000, July 8, 1907–June 25, 1915. (U. S. Office of Indian Affairs.) Washington, D.C.: Government Printing Office, 1916. 39 p. DLC—E93.U723

841. Subject. [76

SWANTON, JOHN R. *The Indian tribes of North America.* (U. S. Bureau of American Ethnology, Bulletin, *145.*) Washington, D.C.: Government Printing Office, 1952. Bib.: 643–82. DLC—E51.U6 no. 145

800 ±. Author. Constitutes a list of references cited in the body of the work. Deals with location, history, and population. [77

TATUM, ROBERT M. "Distribution and bibliography of the petroglyphs of the United States," *American Antiquity,* **12**, *3*, 1946: 125. DLC—E51.A52 vol. 12

45 ±. Author. [78

TERNEAUX-COMPANS, HENRI. *Bibliothèque américaine . . . jusqu'à l'an 1700.* Paris: A. Bertrand, 1837. 191 p. DLC—Z1203.T321

1,153. Chron. Includes historical works,

with many items relating to Indians. Titles of non-French items translated into French. [79

TOOMEY, T. N. *Bibliographies of lesser North American linguistic families.* (Hervas Laboratories of American Linguistics, Bulletin, *1.*) St. Louis: Hervas Laboratories, 1917. 20 p. DLC—PM101. H4 no. 1; Z7118.T66

200 ±. Language, author. Brief annotation. [80

VAIL, ROBERT W. G. "A bibliography of North American frontier literature, 1542–1800," *The voice of the old frontier* (Philadelphia: Univ. of Pennsylvania Press, 1949) : 84–492. DLC—Z1249.F9V3

1,300 ±. Chron. Index: author and subject. Annotated. Location of items indicated. [81

VOEGELIN, CARL F. "North American Indian languages still spoken and their genetic relationships," in LESLIE SPIER, A. IRVING HALLOWELL, and STANLEY S. NEWMAN (eds.), *Language, culture and personality; essays in memory of Edward Sapir* (Menasha, Wisconsin: Sapir Memorial Publication Fund, 1941): 15–40. Bib.: 32–40. DLC—E98.C9L25

170 ±. Author. [82

WAGNER, GÜNTER. "Nordamerika," in "Völkerkundliche Bibliographie von 1928 an, mit Nachträgen der Jahre 1924–1927," *Ethnologischer Anzeiger,* **3,** *1–2,* 1932–34: 8–48. CU—GN1.E75 vol. 3

836. Area, author. Chiefly 1928–31, with a few earlier items. [83

WINSOR, JUSTIN. *Aboriginal America,* vol. 1 of *Narrative and critical history of America.* Boston and New York: Houghton, Mifflin, 1889. Bib.: 413–44. DLC—E18. W76 vol. 1

700 ±. Subject (I—aboriginal America II—American antiquities; III—industries and trade of the American aborigines; IV—American linguistics; V—myths and religions; VI—archaeological museums and periodicals). Running annotation with bib. references *passim.* [84

WOLFF, HANS. "Bibliography of bibliographics of North American Indian languages still spoken," *International Journal of American Linguistics,* **13,** *4,* 1947: 268–73. DLC—PM101.I5 vol. 13

References to 12 bibliographies and to volumes of the *International Journal of American Linguistics* and *Language.* Index: language. [85

WORMINGTON, H. MARIE. *Ancient man in North America.* 4th rev. ed. Denver, Colorado: Denver Museum of Natural History (Popular Series, *4*), 1957. 322 p. Bib.: 281–309. DSI—507.73.C7C69 1957

600 ±. Author. [86

1.11 Alaska, Canada

"Bibliography of Canadian anthropology," *Canada National Museum Annual Report, 1954–55+* (Canada National Museum, Bulletin, *142+*). Annual. DLC—QH.C13

150–180 ±. Author. Annotated. Compiled by T. F. McIlwraith. Continues "Recent publications relating to Canada: ethnology, anthropology and archeology," formerly published in the *Canadian Historical Review.* [87

FRIED, JACOB (ed.). *A survey of the aboriginal populations of Quebec and Labrador.* (Eastern Canadian Anthropological Series, *1.*) Montreal: McGill Univ., 1955. Bib.: 4–52. Map. DLC—E78.Q3F7

500 ±. Cross-classified: 1) author, 2) subject (tribe or area), author. A "Survey of bibliography," p. 53–64, arranged geographically or by linguistic group, provides a further guide to the listings and to unpublished work. An appendix, "Sources of information," p. 117–19, lists many individuals and organizations in possession of records and unpublished materials. [88

GARIGUE, PHILIP. *A bibliographical introduction to the study of French Canada.* Montreal: McGill Univ., Dept. of Sociology and Anthropology, 1956. 133 p. DLC—Z1377.F8G29

2,984. Subject, topic, author. Includes "social institutions" (1,230 items) and "cultural characteristics" (539 items). [89

HODGE, FREDERICK W. (ed.). *Handbook of Indians of Canada.* (Tenth report of the Geographic Board of Canada, Appendix II.) Ottawa: Parmelee, 1913. Bib.: 550–93. DLC—E78.C2H6

2,600 ±. Author. Being a verbatim reprint from the *Handbook of American Indians North of Mexico,* this includes many items which do not deal with Canadian Indians. [90

PILLING, JAMES C. *Bibliography of the Eskimo language.* (U. S. Bureau of Ethnology, Bulletin, *1.*) Washington, D.C.: Government Printing Office, 1887. 116 p. DLC—E51.U6 no. 1

700 ±. Author or translator, interlisted with cross-references by vernacular title, subject, place, and dialect. Index: chron. Annotated. Facsimiles of title pages of some items. [91

POPHAM, ROBERT E. "A bibliography and historical review of physical anthropology in Canada: 1848–1949," *Revue Canadienne de Biologie,* **9,** *2,* 1950: 175–98. DLC—QH301.R47 vol. 9

——— Reprint, *Yearbook of Physical Anthropology 1950,* **6** (New York: Wenner-Gren Foundation for Anthropological Research, Inc., 1951) : 161–84. DLC—GN60.Y4 vol. 6

270 ±. Area, author. [92

"Recent publications relating to Canada: ethnology, anthropology and archeology," *Canadian Historical Review,* **1–35,** 1920–55. Annual. DLC—F1001.C27

150–200 ±. Author. Annotated. Compiled by C. M. Barbeau, D. Jenness, and T. F. McIlwraith. Continued as "Bibliography of Canadian anthropology," *Canada National Museum Annual Reports,* 1954–55+ [93

TREMAINE, MARIE (ed.). *Arctic bibliography.* (U. S. Dept. of Defense, Arctic Institute of North America.) Washington, D.C.: Government Printing Office. 1+ 1935+. DLC—Z6005.P7A7

Over 38,000 in vols. 1–6, 1953–56. Author. Index: author, subject, place. Vol. 3 indexes vol. 1–2; vol. 4+ have own indexes. Extensive annotation. [94

1.12 Eastern U.S.A.

ANDERSON, ROBERT B. "A preliminary check list of the laws of the Indian tribes," *Law Library Journal,* **34,** *4,* 1941: 126–48. DLC—Law Library

350 ±. Tribe, date. Pertains to laws recorded in legislative sessions 1808–1905 for various tribes of eastern United States. [95

Bibliography on Seminole Indians. (N. Y. A. Project.) Gainesville: Univ. of Florida, 1940. 24 p. Processed. DLC—Z1210.S5F6

130. Whole works and references in works on other subjects, author. [96

GRIFFIN, JAMES B. (ed.). *Archeology of eastern United States.* Chicago: Univ. of Chicago Press, 1952. Bib.: 371–92. DLC—E53.G7

840 ±. Author. [97

JOHNSON, FREDERICK (ed.). *Man in northeastern North America.* (Robert S. Peabody Foundation for Archaeology, Papers, **3.**) Andover, Mass.: 1946. Bib.: 309–47. DLC—E51.P56 vol. 3; E71.J6

760 ±. Author. [98

MORGAN, RICHARD G., and JAMES H. RODABAUGH. *Bibliography of Ohio archaeology.* Columbus, Ohio: The Ohio State Archaeological and Historical Society, 1947. 189 p. DLC—Z1323.M6

1,351. Author. Index: subject, county, site name, place, and culture. Brief annotation. " . . . intended to include all published materials relating to Ohio archaeology, except references in newspapers, county and regional histories, and atlases." [99

PIERCE, ROY G. *Bibliographical list of articles, books and references to Virginia Indians, with particular emphasis on archeology.* Richmond, Virginia: 1+, 1944+. Processed. (No. 1 called "preliminary.") DLC—Z1209.A73

125–180 ±. Author. Some items briefly annotated. Location of copies given. Six issues had been published by 1956, each dealing with items compiled in the preceding two to four year period. [100

PILLING, JAMES C. *Bibliography of the Iroquoian languages.* (U. S. Bureau of Ethnology, Bulletin, *6.*) Washington, D.C.: Government Printing Office, 1888. 208 p. DLC—E51.U6 no. 6

1,100 ±. Author or translator, interlisted with cross-references by vernacular title, subject, and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of a few authors. [101

PILLING, JAMES C. *Bibliography of the Muskhogean languages.* (U. S. Bureau of Ethnology, Bulletin, *9.*) Washington, D. C.: Government Printing Office, 1889. 114 p. DLC—E51.U6 no. 9

521. Author or translator, interlisted with cross-references by vernacular title, subject, and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. [102

ROUSE, IRVING, and JOHN M. GOGGIN (eds.). *An anthropological bibliography of the eastern seaboard.* (Eastern States Archeological Federation, Research Publication, *1.*) New Haven: Yale Peabody Museum, 1947. 174 p. DLC—E51.E212 no. 1

3,200 ±. Subject, area, author. Includes maps. Supersedes IRVING ROUSE and DONALD HORTON, "Preliminary archeological bibliography of the eastern United States," *Bulletin of the Archeological Society of Connecticut, 9, 10:* 1939–40 [103

SWANTON, JOHN R. *Indians of the Southeastern United States.* (U. S. Bureau of American Ethnology, Bulletin, *137.*) Washington, D.C.: Government Printing Office, 1946. Bib.: 832–56. DLC—E51.U6 no. 137

350 ±. Author. Preceded by a discussion of source materials, p. 827–32. [104

THOMAS, VIOLA L. D. (MRS. CYRUS). "Bibliography of the earthworks of Ohio," *Ohio Archaeological and Historical Quarterly,* **1**, 1887–88: 69–78, 191–200, 272–82. DLC—F486.051 vol. 1

280 ±. Brief annotation. References follow the list of locations of earthworks. [105

WINSOR, JUSTIN. *The New England Indians: a bibliographical survey, 1630–1700.* Cambridge, Mass.: J. Wilson and Son, 1896. 35 p. Bib.: *passim.* DLC—Z1209.W78

—— "The New-England Indians," an abbreviated version of the above, appeared in *Proceedings of the Massachusetts Historical Society,* second series, **10**, 1895–96: 327–59. DLC—F61.M38 1895–96

100 ±. Chron. Annotated, including location of copies. [106

1.13 Western U.S.A.

ABEL, ANNIE H. "The history of events resulting in Indian consolidation west of the Mississippi," *American Historical Association Annual Report . . . 1906* (Washington, D. C.: 1908). "Bibliographic guide to the primary and secondary authorities," **1**: 413–54. DLC—E172.A60 1906 vol. 1

325 ±. Author. Index: author and subject. Extensive annotation. [107

BANCROFT, HUBERT H. *The native races of the Pacific states of North America.* New York: Appleton, 1875–76. 5 vols. Bib., **1**: xvii–xlix. DLC—E58.B215 vol. 1

1,150 ±. Author or title. [108

BEALS, RALPH L. *Ethnology of Rocky Mountain National Park; the Ute and the Arapaho.* Berkeley, Calif.: U. S. National Park Service, 1935. Processed. Bib.: 20–27. DLC—E99.U8U57

80 ±. Author. [109

BEALS, RALPH L. *Preliminary report on the ethnography of the Southwest.* Berkeley, Calif.: U. S. National Park Service, 1935. Processed. Bib.: 1–36 (separately paginated, at end). DLC—E78.S7U85

450 ±. Author. [110

Bibliography, Mesa Verde National Park, Colorado. (Mesa Verde National Park, Colorado, Occasional Papers, *1.*) 1935? 20 p. Processed. DLC—Z1264.M5U5

250 ±. Author. Some items briefly annotated. [111

"Bibliography of New Mexican archaeology, 1936 and 1937, with résumé of field work," *New Mexico Anthropologist* (Albuquerque), **2**, *2*, 1937: 36–44. DLC—E77.N45 vol. 2

110 ±. Author. Continued in "Bibliography of southwestern archaeology, January 1938–April 1939," *New Mexico Anthropologist,* **3**, *3–4*, 1938–1939: 44–48. 110 ±. Author. [112

"Bibliography of New Mexican ethnology and ethnography, 1936 and 1937, with résumé of southwestern field work," *New Mexico Anthropologist* (Albuquerque) **2**, *3*, 1938: 52–62. DLC—E77.N45 vol. 2

165 ±. Author. Continued in "Bibliography of southwestern ethnology, Jan. 1938–April 1939," *New Mexico Anthropologist,* **3**, *3–4*, 1938–39: 49–53. 110 ±. Author. [113

BRAND, DONALD D., and others. "Chaco Canyon bibliography," *Tseh So.* (University of New Mexico, Bulletin, *308;* Anthropological Series, **2**, *2.*) Albuquerque, New Mexico: 1937, p. 120–33. DLC—F798.N392 vol. 2 no. 2

260 ±. Author. Items of greater importance starred. "All original published accounts of archaeological work in Chaco Canyon have been listed, as well as many secondary and popular accounts." Supplemented by CLYDE KLUCKHOHN, "Addenda to Chaco Canyon bibliography." [114

CAMPBELL, THOMAS N. *A bibliographic guide to the archaeology of Texas.* (Univ. of Texas, Dept. of Anthropology, Archaeology Series, *1.*) Austin, Texas: 1952. 64 p. DLC—F388.T4 no. 1.

689. Author. Index ("Guide to the literature"): subject, cultural unit or area. [115

EWERS, JOHN C. *Teton Dakota, ethnology and history.* Rev. ed. Berkeley, Calif.: U. S. National Park Service, 1938. Processed. Bib.: 99–108. DI—E99.T34U5 1938

210 ±. Author. [116

GUERBER, LOUISE. *Cliff dwellers and cliff dwellings of North America; a selected bibliography.* Denver: Public Library, 1925. 21 p. DLC—Z1209.D355

200 ±. Books, government documents, articles; author. [117

HEIZER, ROBERT F. *A bibliography of ancient man in California.* (California Archaeological Survey, Reports, *2.*) Berkeley, Calif.: Univ. of Calif., Dept. of Anthropology, 1948. 22 p. DLC—F863.C255 no. 2

185. Site or subject, author. Each site or subject bears introductory remarks; some items annotated. [118

JENNINGS, JESSE D. *The archeology of the Plains: an assessment.* Salt Lake City, Utah: National Park Service and Univ. of Utah, 1955. Bib.: 140–80; David Dibble, comp. UU—571.136 J54a

863. Author. Concerns particularly Missouri River Basin archaeology since 1935. [119

KLUCKHOHN, CLYDE. "Addenda to Chaco Canyon bibliography," Appendix C in CLYDE KLUCKHOHN and PAUL REITER (eds.), *Preliminary report on the 1937 excavations . . . Chaco Canyon, New Mexico.* (University of New Mexico, Bulletin, *345;* Anthropological Series, **3**, *2.*) Albuquerque, New Mexico: 1939, p. 175–76. DLC—F798.N392 vol. 3, no. 2

34. Author. Supplements DONALD D. BRAND and others, "Chaco Canyon Bibliography." [120

KLUCKHOHN, CLYDE, and KATHERINE SPENCER. *A bibliography of the Navaho Indians.* New York: Augustin, 1940. 93 p. DLC—Z1210.N3K7

2,000 ±. Subject, topic, author. Index: author. Some items annotated. [121

KROEBER, ALFRED L. *Handbook of the Indians of California.* (U. S. Bureau of American Ethnology, Bulletin, *78.*) Washington, D.C.: Government Printing Office, 1925. Bib.: 954–68. DLC—E51.U6 no. 78.

—— —— Reprint. Berkeley, Calif.: Calif. Book Co., Ltd., 1953.

415. Author. Index: subject. Many items with brief annotation. [122

LUOMALA, KATHERINE. *Navaho life yesterday and today.* Berkeley, Calif.: U. S. National Park Service, 1938. Processed. Bib.: 104–15. DLC—E99.N3U6

180 ±. Author. [123

PILLING, JAMES C. *Bibliography of the Chinookan languages (including the Chinook jargon).* (U. S. Bureau of Ethnology, Bulletin, *15.*) Washington, D.C.: Government Printing Office, 1893. 81 p. DLC—E51.U6 no. 15

270. Author or translator, interlisted with cross-references by subject and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. Facsimiles of selected pages. [124

PILLING, JAMES C. *Bibliography of the Salishan languages.* (U. S. Bureau of Ethnology, Bulletin, *16.*) Washington, D.C.: Government Printing Office, 1893. 86 p. DLC—E51.U6 no. 16

320. Author or translator, interlisted with cross-references by subject and tribe. Index: chron. Annotated, some items extensively. Biographical sketches of some authors. Facsimiles of selected pages. [125

RADER, JESSE L. *South of forty, from the Mississippi to the Rio Grande, a bibliography.* Norman: Univ. of Oklahoma Press, 1947. 336 p. DLC—Z1251.S83R3

3,793. Author. Index: subject (includes tribes, Indian history, antiquities, myths and legends, etc.). Brief annotations. [126

REED, ERIK K. "Bibliography of the archaeology of the Jémez Mountain area, New Mexico, U. S. A," *B. B. A. A.,* **3**, *1*, 1939: 16–21. DLC—Z5112.P3 vol. 3

83. Region, author. [127

REED, ERIK K. "Bibliography of the Mimbres Valley and the Mogollon culture," *B. B. A. A.,* **3**, *2*, 1939: 125–33. DLC—Z5112.P3 vol. 3

85. Author. Some items briefly annotated. [128

SAUNDERS, LYLE. *A guide to materials bearing on cultural relations in New Mexico.* Albuquerque: Univ. of New Mexico Press, 1944. 528 p. DLC—Z1315.S35

5,335. Tribe or people and historical period, author. Indexes: author, subject. Some brief annotation. Bibliography preceded by a "dictionary-guide" indexing 263 selected titles. [129

STARR, FREDERICK. "Bibliography of Iowa antiquities," *Proceedings of the Davenport Academy of Natural Sciences,* **6**, 1892: 1–24. DLC—QH1.D2 vol. 6

205. Author. Brief annotation. [130

STEWARD, JULIAN. *The Blackfoot.* Berkeley, Calif.: U. S. National Park Service, 1934. Processed. Bib. (separately paginated at end), 15 p. DLC—E99.S54U75

160 ±. Author. "Includes all those sources which give the important ethnographic facts about the Blackfoot." [131

WAGNER, HENRY R. *The plains and the Rockies, a bibliography of original narratives of travel and adventure, 1800–1865.* 3d ed. rev. by CHARLES L. CAMP. Columbus, Ohio: Long's College Book Store, 1953. 601 p. and pl. DLC—Z1251.W5W2 1953

429. Chron. Extensive annotation. Index: author, subject, and place; includes "Indian affairs" 24 items, various tribes by name. [132

1.2 Latin America

"Anthropology," *H. L. A. S.*, 1936+ Annual. DLC—Z1605.H23

150–1,000 ±. Subject, area, author. Annotated. Indexes: author and subject, at ends of volumes. Compiled by A. M. Tozzer, R. Redfield, W. C. Bennett, A. Métraux, T. D. Stewart, G. C. Vaillant, R. L. Beals, G. F. Ekholm, D. Collier, G. R. Willey, D. B. Stout, S. Tax, R. Wauchope, J. H. Rowe, I. Rouse, A. Palerm, S. W. Mintz, and others. [133

BOGGS, RALPH S. *Bibliography of Latin American folklore.* (Inter-American Bibliographical and Library Association, Publications, Ser. 1, 5.) New York: Wilson, 1940; Washington, D.C.: 1940. 109 p. DLC—Z5984.L4B7; Z5984.L4B7 1940a

643. Subject, country, author. Index: author and topic. Some items annotated. A selected bibliography. [134

BOSSIO, NÉLIDA. "La indagación serológica en Sudamérica," *B. B. A. A.*, **9**, 1947: 71–77. DLC—Z5112.P3 vol. 9

26. Chron. Annotated. [135

"Cartography," *H. L. A. S.*, **1–12**, 1935–46. Annual. DLC—Z1605.H23

Each issue lists 2–10 maps of archaeological or ethnological nature. Indexes: author and subject at end of each volume. Annotated. Compiled by Raye R. Platt. In later volumes anthropological maps are included in the anthropology section. [136

COMAS, JUAN. *Bibliografía morfológica humana de América del sur.* (Instituto Indigenista Inter-americano, Ediciones Especiales, 7.) Mexico: 1948. vol. 1, 208 p. DLC—Z7994.M8C6

2,971. Author. Indexes: subject, tribe, place. Pertains to continental South America exclusively. Vol. 2 contains maps. [137

The fine and folk arts of the other American republics. A bibliography of publications in English. (U. S. Library of Congress, Hispanic Foundation: The Archive of Hispanic Culture.) Washington, D.C.: 1942. 18 p. DLC—Z5961.S72U5

187. Subject, topic. Includes archaeology. [138

"Folklore," *H. L. A. S.*, **2–11**, 1936–45. Annual. DLC—Z1605.H23

30–200 ±. Subject, area, author. Indexes: author and subject, at end of each volume. Compiled by Ralph S. Boggs. [139

GUSINDE, MARTIN. "Südamerika," in "Völkerkundliche Bibliographie von 1928 an, mit Nachträgen der Jahre 1924–1927," *Ethnologischer Anzeiger*, **3**, *1–2*, 1932–34: 73–118. CU—GN1.E75 vol. 3

900. Area, author. Chiefly 1928–31, with a few earlier items. [140

Ibero-Amerikanische Bibliographie. Auswahlverzeichnis der deutsch-sprachigen Literatur. **1–57**, 1930–45. (Ibero-Amerikanisches Archiv, Beilage, 4, *2* to **18**, *2*.) Quarterly. DLC—Z1605.I13

400–500 ±. Area, subject, author. Pertains to Spanish and Portuguese culture in America and other parts of the world. Compiled by Hans Praesent. [141

JIMÉNEZ MORENO, WIGBERTO. "Materiales para una bibliografía etnográfica de la América latina," *B. B. A. A.*, **1**, 1937–38: 47–77, 167–97, 289–421. DLC—Z5112.P3 vol. 1

—— Reprint. Mexico: Instituto Panamericano de Geografía e Historia. DLC—Z5114.J5

3,000 ±. Major area, sub-area or tribe, author. Some items annotated. [142

JONES, CECIL K. *A bibliography of Latin American bibliographies.* 2nd ed. (U. S. Library of Congress, Hispanic Foundation) Washington, D. C.: Government Printing Office, 1942. 276 p. DLC—Z1601.A2J7 1942

3,016. Country, author. Index: author, subject, country, place. Some items annotated. Subjects indexed include anthropology, ethnology, archaeology, Indian languages. [143

LATCHAM, RICARDO E. "Breve bibliografía de los petroglifos sudamericanos," *Revista de Bibliografía Chilena*, **1**, 1927: 42–49. DLC—Z1701.R4 vol. 1

127. Author. Brief annotation. [144

MARCHANT, ALEXANDER, and CHARMION SHELBY (eds.); JOHN E. ENGLEKIRK (advisory ed.). *Investigations in progress in the United States in the field of Latin American humanistic and social science studies.* Preliminary edition. (U. S. Library of Congress, Hispanic Foundation.) Washington, D.C.: 1942. 236 p. DLC—AZ505.A5

Lists of persons active on Nov. 1, 1941, their principal research interests, principal publications (1,100 ±), and works in progress. Indexes: discipline, and region subdivided by discipline. No later edition has appeared. [145

MONTIGNANI, JOHN B. *Books on Latin America and its art in the Metropolitan Museum of Art library.* New York: 1943. 63 p. Processed. DLC—Z5961.S72N47

480 ±. Subject, area, topic, author. Brief annotation of some items. [146

MURDOCK, GEORGE P. *Outline of South American cultures.* (Behavior Science Outlines, *2*.) New Haven: Human Relations Area Files, 1951. Bib.: *passim*, 148 p. DLC—D16.25.B44

500 ±. Geog. Index: tribes and modern nations. Deals with southern Central America and the Antilles as well as with continental South America. [147

SOBRÓN, FÉLIX C. Y. *Los idiomas de la América latina; estudios biográfico-bibliográficos.* Madrid: Saiz, 1875? 139 p. DLC—PM5005.S6

200 ±. Area, author. Annotated. Includes the Philippine Islands and Vavau (Tonga). [148

STEWARD, JULIAN H. (ed.). *Handbook of South American Indians.* (U. S. Bureau of American Ethnology, Bulletin *143*, **1–6**.) Washington, D.C.: Government Printing Office, 1946–50. References at ends of articles, combined bibs. at ends of vols. DLC—E51.U6 no. 143: F2229.S75 no. 143

500–3,030 ±. Author. Deals with Central America and the Antilles (vol. 4) as well as with continental South America. [149

STEWART, T. DALE. *A bibliography of physical anthropology in Latin America: 1937–1948.* New York: Wenner-Gren Foundation, 1952. 59 p. DLC—Z5118.S7S83

468. Author. Index: subject. Annotated. Combines the author's annual bibliographies which appeared in *H. L. A. S.*, **4–14**, 1938–48. [150

1.21 Mexico and Central America

BANDELIER, ADOLPH F. "Notes on the bibliography of Yucatan and Central America," *Proceedings of the American Antiquarian Society*, **n. s. 1**, 1880: 82–118. DLC—E172.A355 vol. 1

—— —— Reprint. Worcester, Mass.: Hamilton, 1881. 39 p. DLC—Z1437.B23

400 ±. Region, chron. Annotated; gives especially various translations and editions of early works. [151

BAYER [BEYER], HERMANN. *Sucinta bibliografía sistemática de etnografía y arqueología Mejicanas.* Mexico: Secretaría de Educación Pública, Dept. Editorial, 1932. 40 p. ICU—Z5114.B57

218. Subject, topic. Some items annotated. [152

BEALS, RALPH L., ROBERT REDFIELD, and SOL TAX. "Anthropological research problems with reference to the contemporary peoples of Mexico and Gautemala," *American Anthropologist*, **45**, *1*, 1943: 1–21. Bib.: 17–21. DLC—GN1.A5 vol. 45

92. Geog., author. Limited to works since 1890; general works where available rather than short articles; few linguistic works. [153

BEAUVOIS, EUGÈNE. "Les publications relatives à l'ancien Mexique depuis une trentaine d'années," *Compte-rendu, Congrès Bibliographique International, Paris, 1898.* Paris: 1900. **1**: 475–97. Bib.: *passim*. DLC—Z1008.C75 1898

300 ±. Subject. Text provides annotation. [154

Bibliografía de las artes populares plásticas de México. (Instituto Nacional Indigenista, Memorias, **1**, *2*: 83–132.) Mexico: 1950. DLC—Z5961.M4I5 vol. 1

480 ±. Author. Some items annotated. Indexes: subject and tribe (full bibliographical information repeated, but annotation omitted). "Prólogo" by Alfonso Caso. [155

BOGGS, RALPH S. "Bibliografía del folklore mexicano," Appendix to *B. B. A. A.*, **3**, *3*, 1939. 122 p. DLC—Z5112.P3 vol. 3

1,223. Subject, author. Some items annotated. Pagination separate from main volume. [156

BOGGS, RALPH S. "Una bibliografía completa, clasificada, y comentada, de los artículos de Mexican Folkways (MF), con índice," *B. B. A. A.*, **6**, 1942: 221–65. DLC—Z5112.P3 vol. 6

245. Subject, author. Index: author and subject. Brief annotation. [157

BRAND, DONALD D. "An historical sketch of anthropology and geography in the Tarascan region, part I," *New Mexico Anthropologist*, **6–7** (combined), *2*, 1943: 37–108. DLC—E77.N45 vol. 6–7

350 ±. Region and type of source. Extensive annotation. Includes published works and manuscripts. [158

BRASSEUR DE BOURBOURG, C. E. *Bibliothèque mexico-guatémalienne* . . . . Paris: Maisonneuve, 1871. Bib.: 1–183. DLC—Z1431.B82

500 ±. Author. Index: native languages. Extensive annotation. Includes manuscripts and published works. Constitutes a catalog of Bourbourg's library. Includes some items relating to North America. [159

BRINTON, DANIEL G. *Catalogue of the Berendt linguistic collection.* (Univ. of Pennsylvania, Free Museum of Science and Art, Bulletin, **2**, *4*.) Philadelphia: 1900. 32 p. DLC—CC21.P45 vol. 2

183. Language. Many items annotated. Lists manuscripts and published works in the Free Museum which deal with the languages of southern Mexico and Central America. [160

CHÁVEZ, ALBERTO N. "Bibliografía antropológica Otomí," *Ethnos* (Mexico), **1**, *3*, 1920: 68–73. DLC—F1201.E84 vol. 1

69. Subject, author. Occasional brief annotation. [161

CLINE, HOWARD. "Mexican community studies," *The Hispanic American Historical Review*, **32**, *2*, 1952. Bib.: 232–42. DLC—F1401.H66 vol. 32

81. Subject, author. Annotated. Includes modern Mexican community studies, marginal community studies, and selected related works, 1923–52. Preceding text provides further annotation. [162

COMAS, JUAN. "Bosquejo histórico de la antropología en México," *Revista Mexicana de Estudios Antropológicos* (Mexico), **11**, 1950: 97–192. Bib.: 142–91, *passim*. DLC—F1201.R46 vol. 11

800 ±. Chron., subject or institution, author. Annotated. [163

CONZEMIUS, EDUARD. "Bibliografía referente a los indios payas de Honduras," *Revista del Archivo y Biblioteca Nacional de Honduras* (Tegucigalpa), **8**, 1929: 279–82, 349–53. DLC—CD3780.A5 vol. 8

60 ±. Author. Annotated. [164

EWALD, ROBERT H. *Bibliografía comentada sobre antropología social guatemalteca 1900–1955*. Guatemala: Seminario de Integración Social Guatemalteca, 1956. Bib.: 81–132. DLC—Z5114.E9

292. Author. Preceding text treats Guatemalan social anthropology by area and topic, and constitutes a guide to and an annotation of the bibliography. [165

GERMÁN PARRA, MANUEL, and WIGBERTO JIMÉNEZ MORENO. *Bibliografía indigenista de Mexico y Centro-América (1850–1950)*. (Instituto Nacional Indigenista, Memorias, *4*.) Mexico: 1954. 342 p. DLC—Z1209.G47

6,445. Subject, author. Indexes: author and institution, place, group and language. [166

GROPP, ARTHUR E. "A bibliography of Totonac linguistic materials," *Hispanic American Historical Review*, **18**, *1*, 1938: 114–26. DLC—R1401.H66 vol. 18.

60 ±. Author. Brief annotation. [167

HORCASITAS PIMENTEL, FERNANDO. "Piezas teatrales en lengua Nahuatl: bibliografía descriptiva," *B. B. A. A.*, **11**, 1948: 154–64. DLC—Z5112.P3 vol. 11

34 manuscripts. Annotation gives name, location, location of photostat or microcopy, editions, translations, author, provenience, period, subject matter, characters, remarks. [168

JIMÉNEZ MORENO, WIGBERTO. "Adiciones a la bibliografía lingüística zapoteca de Peñafiel," in his "Introducción" to FRAY JUAN DE CÓRDOVA, *Vocabulario castellano-zapoteco (edición facsimilar)*. Mexico: Instituto Nacional de Antropología e Historia, 1942. p. 33–37. DLC—PM4548.J8 1942

48. Author. Some items annotated. [169

JIMÉNEZ MORENO, WIGBERTO. "Pequeña bibliografía etnográfica de México," *Boletín del Museo Nacional de Arqueología, Historia y Etnografía*, Época 6, **1**, *2*, 1934. Mexico: 1937. p. 125–37. DSI—507.72 ser. 6, vol. 1

180 ±. Tribal group or area. [170

JOHNSON, FREDERICK. "The linguistic map of Mexico and Central America," in *The Maya and their neighbors*. New York: Appleton-Century, 1941. p. 88–114. Bib.: *passim*. DLC—F1435.M45

90 ± references to bib. at end of volume. [171

KUBLER, GEORGE, and CHARLES GIBSON. *The Tovar calendar; an illustrated Mexican manuscript* ca. *1585, reproduced with a commentary and handlist of sources on the Mexican 365-day year.* (Connecticut Academy of Arts and Sciences, Memoir **11**.) New Haven: 1951. 82 p. and pl. "Handlist": 55–78; bib.: 79–82. DLC—Q11.C85 vol. 11

175 ± in bib., 76 codices and other documents in handlist. Items in handlist extensively annotated. [172

LABASTILLE, IRMA G. "The music of Mexico and Central America," *H. L. A. S.*, 1936: 459–72. DLC—Z1605.H23 1936

190 ±. Area, author. [173

LEHMANN, WALTER. "Ergebnisse und Aufgaben der mexikanistischen Forschung," *Archiv für Anthropologie*, n.s. **6**, *2–3*, 1907: 113–68. Bib.: *passim*. DLC—GN2.D32

——— Translation by SEYMOUR DE RICCI. *Methods and results in Mexican research*. Paris: Herbert Clarke, 1909. 127 p. DLC—F1219.L51

600 ±. Sections on bibliography, sources, general works, anthropology, linguistics, ethnology, and history. [174

LEJEAL, LÉON. *Les antiquités mexicaines (Mexique, Yucatan, Amérique-centrale)*. (Société des Études Historiques, Bibliothèque de Bibliographies Critiques, *19*.) Paris: Picard, 1902. 79 p. DLC—Z1009.B55

388. Subject, author. Index: author. Annotated. [175

LEÓN, NICOLÁS. *Apuntes para una bibliografía antropológica de México (somatología)*. Mexico: Museo Nacional, 1901. 18 p. DLC—Z5114.L57

167. Author. Some items annotated. [176

LEÓN, NICOLÁS. "Bibliografía antroposomatológica Mexicana," in "La antropología física y la antropometría en México," *Anales del Museo Nacional de Arqueología, Historia y Etnografía* (Mexico). Época 4, **1**, *2*, 1922: 118–36. DLC—F1219.M7 vol. 1

308 ±. Author. [177

LEÓN PORTILLA, MIGUEL, and SALVADOR MATEOS HIGUERA. Catálogo de los códices indígenas del México antiguo. (Suplemento del Boletín Bibliográfico de la Secretaría de Hacienda, año 3, *111*.) Mexico: June, 1957. 53 p. DPU—Z6605.M6L4

61. Culture, era, title. Extensive annotation, including location of the original, physical description, content, and list of reproductions for each. Appendix lists libraries and museums in which codices are deposited. For each culture, other codices not herein described are listed, together with their locations. [178

LINES, JORGE A. *Bibliografía antropológica aborigen de Costa Rica*. San José, Costa Rica: Univ. de Costa Rica, 1943. 263 p. DLC—Z5114.L63

1,262. Author. Includes manuscripts. Some items briefly annotated. [179

List of works in the New York Public Library relating to Mexico. New York (City): Public Library, 1909. 186 p. DLC—Z1431.N56

5,500 ±. Subject and author. Some items annotated (especially public documents). Subjects include archaeology and Indians, languages, folklore, etc. [180

"Lista bibliográfica mexicana 1929–1931," *Boletín del Museo Nacional de Arqueología, Historia y Etnografía* (Mexico), Época 5, **1** [misnumbered **3**], *2*, 1932: 46–58. DSI—507.72 1932

210 ±. Subject, author. Sections on anthropology, archaeology, ethnography, linguistics. [181

MALDONADO-KOERDELL, MANUEL. "Bibliografía mexicana de prehistoria," *B. B. A. A.*, **9**, 1946: 66–71; **10**, 1947: 98–102; **11**, 1948: 148–54. DLC—Z5112.P3

240 ±. Subject, topic, author. [182

MALDONADO-KOERDELL, MANUEL. "Estudios etnobiológicos, notas a una bibliografía mexicana de botánica," *B. B. A. A.*, **6**, 1942: 61–74. DLC—Z5112.P3 vol. 6

15. Extensive annotation. [183

MARINO FLORES, ANSELMO. "Bibliografía antropológica del estado de Guerrero," *B. B. A. A.*, **15–16**, Pt. 1, 1952–53: 233–89. DLC—Z5112.P3 vol. 15–16

552. Author. Some items annotated (contents listed). [184

MARINO FLORES, ANSELMO. *Bibliografía lingüística de la república mexicana*. Mexico: Instituto Indigenista Interamericano, 1957. Bib.: 17–95; 2 maps. DPU—Z7120.M3

1,100 ±. Language, author. Bib. preceded by a list of native speakers ordered by Mexican states, and a map of indigenous languages by municipios. [185

MARINO FLORES, ANSELMO. "Contribución a una bibliografía antropológica sobre los Tarascos—México," *B. B. A. A.*, **8**, 1945: 80–100. DLC—Z5112.P3 vol. 8

366. Author. [186

MASON, J. ALDEN. "The native languages of Middle America," in *The Maya and their neighbors*. New York: Appleton-Century, 1941. p. 52–87. Bib.: *passim*. DLC–F1435.M45

100 ±. text references to bib. at end of volume. [187

MCAFEE, BYRON. "Bibliografía de gramáticas y métodos de lengua Nahuatl que poseo," *Investigaciones Lingüísticas* (Mexico), **3**, *3–4*, 1935: 227–30. DLC–PM3001.I5 vol. 3

50 ±. Chron. [188

MORLEY, SYLVANUS G. *The ancient Maya*. 3d ed., rev. by GEORGE W. BRAINERD. Stanford, Calif.: Stanford Univ. Press, 1956. 494 p. Bib.: 467–81. (1st edition 1946.) DLC–F1435.M75 1956

196. Author. [189

PEÑAFIEL, D. ANTONIO. "Notas bibliográficas. Escritores que se han ocupado de la lengua zapoteca," *Gramática de la lengua zapoteca*. Mexico: Sec. de Fomento, 1887. p. v–lvii. DLC–PM4547.G7

106. Bibliographies and primary sources, author. Index: author (a tabular résumé). Annotated, including biographical sketches of authors. See also WIGBERTO JIMÉNEZ MORENO, "Adiciones a la bibliografía lingüística zapoteca de Peñafiel." [190

ROYS, RALPH L. *The Indian background of colonial Yucatan*. (Carnegie Institution of Washington, Publication, *548*.) Washington, D.C.: 1943. 244 p. Bib.: 199–212. DLC–F1219.1.Y8R6; AS32.A5 no. 548

240 ±. Author. [191

SAVILLE, MARSHALL H. *Bibliographic notes on Palenque, Chiapas*. (M. A. I., H. F., Indian Notes and Monographs, **6**, *5:* 119–80.) New York: 1928. DLC–E51.N45 vol. 6 no. 5

232. Chron. Index: author. Extensive annotation of most items. [192

SAVILLE, MARSHALL H. *Bibliographic notes on Quirigua*. (M. A. I., H. F., Indian Notes and Monographs, **6**, *1*.) New York: 1919. 22 p. DLC–E51.N45 vol. 6 no. 1

49. Chron. Annotated. [193

SAVILLE, MARSHALL H. *Bibliographic notes on Uxmal, Yucatan*. (M. A. I., H. F., Indian Notes and Monographs, **9**, *2:* 55–131.) New York: 1921. DLC–E51.N45 vol. 9 no. 2

92. Chron. Index: author. Annotated, including long extracts from the earlier items. [194

SAVILLE, MARSHALL H. *Bibliographic notes on Xochicalco, Mexico*. (M. A. I., H. F., Indian Notes and Monographs, **6**, *6:* 190–207.) New York: 1928. DLC–E51.N45 vol. 6 no. 6

47. Chron. Index: author. Annotated. [195

SAVILLE, MARSHALL H. *Check-list of recent publications on Mayan archeology*. (M. A. I., H. F., Indian Notes, **4**, *4:* 370–84.) New York: 1927. DLC–E51.N44 vol. 4 no. 4

108. Author. Few items annotated. Concerns items published 1924–27. [196

SAVILLE, MARSHALL H. "Mexican codices: a list of recent reproductions," *American Anthropologist*, n.s., **3**, *3*, 1901: 532–41. DLC–GN1.A5 vol. 3

43 ±. Chron. Brief annotation. [197

SQUIER, EPHRAIM G. *Monograph of authors who have written on the languages of central America* . . . . New York: Richardson, 1861. 70 p. DLC–Z7120.S772. Also London: Trubner, 1861. DLC–Z7120.S77

220 ±. Author. Indexes: tribe and language. Includes short biographies. Annotated. Appendix lists works on history and aborigines of Central America. [198

STARR, FREDERICK. *Recent Mexican study of the native languages of Mexico*. (Chicago University, Dept. of Anthropology, Bulletin, 4.) Chicago: University of Chicago Press, 1900. 19 p. DLC–F1219.C58 no. 4; Z7120.S85

75. Type of work (ancient manuscripts and old works, recent linguistic studies, vernacular works), author. Annotated, some items at length. Includes manuscripts. Constitutes an addition to JOAQUÍN GARCÍA ICAZBALCETA, *Apuntes para un catálogo de escritores en lenguas indígenas de América*. [199

STEWARD, JULIAN H. (ed.). *Handbook of South American Indians; vol. 4, the circum-Caribbean tribes*. (U. S. Bureau of American Ethnology, Bulletin, *143*, **4**.) Washington, D.C.: Government Printing Office, 1948. References at ends of articles, combined bib. at end of volume. DLC–E51.U6 no. 143, vol. 4; F2229.S75 no. 143, vol. 4

500 ±. Author. Honduras and south. [200

STONE, DORIS. "A report on folklore research in Costa Rica," *Jour. of American Folklore*, **64**, *251*, 1951: 97–103. DLC–GR1.J8 vol. 64

100 ±. Indigenous and non-Indian, subject, author. [201

STONE, DORIS. "A report on folklore research in Honduras," *Jour. of American Folklore*, **64**, *251*, 1951: 113–20. DLC–GR1.J8 vol. 64

80 ±. Indian, non-Indian, poetry, drama; author. [202

TEIXIDOR, FELIPE. *Bibliografía yucateca*. (Museo Arqueológico e Historico de Yucatan, Ediciones, *1*.) Mérida: Talleres gráficos del Sudeste, 1937. 263 p. DLC–Z1427.Y8T2

1,737. Subject and author. Some items annotated. Part 1: Maya language, 239 items; part 2: historical manuscripts, 38 items; part 3: literature, history, science, 1,460 items. Ethnology, archaeology, physical anthropology, etc., dispersed in part 3. [203

TOZZER, ALFRED M. (ed.). *Landa's relación de las cosas de Yucatan*. (Harvard University, Peabody Museum of American Archaeology and Ethnology, Papers, **18**.) Cambridge, Mass.: 1941. Bib.: 337–64. DLC–E51.H337 vol. 18; F1376.L248

670 ±. Author. Concerns the Maya at the time of first European contact. [204

TOZZER, ALFRED M. *A Maya grammar, with bibliography and appraisement of the works noted*. (Harvard Univ., Peabody Museum of American Archaeology and Ethnology, Papers, **9**.) Bib.: 211–79. DLC–E51.H337 vol. 9; PM3963.T6

650 ±. Author. Part III of the preceding text constitutes a subject index and annotation of the bibliography. Intended to be "as complete a list as possible" of works "which describe or treat the language at some length." [205

TRISTÁN, JOSÉ M. *Bibliografía maya yucateca*. Rochester, New York: 1949. 125 p. Processed. DLC–Z1210.M4T73

1,000 ±. Author. Some items annotated. [206

UGARTE, SALVADOR. *Catálogo de obras escritas en lenguas indígenas de México (o que tratan de ellas de la biblioteca particular de Salvador Ugarte)*. 2d ed. México: Jus, 1954. 221 p. DLC–Z7120.U35

431 published works, 8 manuscripts. Author. Index: language. Annotated. Reproductions of some title pages. [207

VAILLANT, GEORGE C. *Aztecs of Mexico: origin, rise and fall of the Aztec nation*. Garden City, N. Y.: Doubleday, Doran, 1941. 340 p. Bib.: 299–325. (Reprinted, Harmondsworth, England: Penguin Books, 1950.) DLC–F1219.V13

360 ±. Author. [208

VALLE, RAFAEL H. "Bibliografía maya," Appendix to *B. B. A. A.*, **1–5**, 1937–41. 404 p. DLC–Z1201.M4V3; Z5112.P3

3,800 ±. Author. Some items annotated, some extensively. Pagination separate from main portions of the volumes. [209

VALLE, RAFAEL H. "El folklore en la literatura de Centro America," *Jour. of American Folk-lore*, **36**, *140*, 1923: 105–34. DLC–GR1.J8 vol. 36

500 ±. Author. Some annotations. [210

VILLACORTA CALDERÓN, J. ANTONIO. "Bibliografía de la lengua maya y de los Mayances guatemaltecos," *Primera Centenario de la Sociedad Mexicana de Geografía y Estadística 1833–1933* (Mexico, 1933): **1**, 71–104. DLC–F1201.S6747 vol. 1

160 ±. Early works, later works by dialect group, kind of work (grammars, dictionaries, vocabularies, catechisms, etc.). Annotation cites original source; early items annotated extensively. [211

WAGNER, G., W. KRICKENBERG, and M. GUSINDE. "Zentralamerika," in "Völkerkundliche Bibliographie von 1928 an, mit Nachträgen der Jahre 1924–1927," *Ethnologischer Anzeiger*, **3**, *1–2*, 1932–34: 49–73. CU–GN1.E75 vol. 3

477. Area, author. Chiefly 1928–31, with few earlier items. [212

YDE, JENS. "Bibliography of the archaeology of Honduras, including the most important geographical works on the country," Appendix III in "An archaeological reconnaissance of northwestern Honduras," *Acta Archaeologica*, **9**, *1–2*, 1938: 91–99. DLC–CC1.A2 vol. 9

——— Reprint: Middle American Research Institute, Tulane University, *Middle American Research Series, 9*. Copenhagen: Levin & Munksgaard, 1938. DLC–F14W1.T95 no. 9

280 ±. Author. [213

1.22 Antilles

BISSAINTHE, MAX. *Dictionnaire de bibliographie haïtienne*. Washington, D.C.: Scarecrow Press, 1951. 1055 p. DLC–Z1531.B5

9,338. Books and journals, area (Haiti and Santo Domingo), author. Indexes: title, subject. Some items annotated. About 37 anthropological listings. [214

HISS, PHILIP H. *A selective guide to the English literature on the Netherlands West Indies. With a supplement on British Guiana*. (Netherlands Information

Bureau. Booklets. *9.*) New York: 1943. 130 p. DLC—Z1502.D7H5

1,100 ±. Region, subject, topic, author. Index: author. Subjects include history and archaeology, social conditions, anthropology, languages. [215

PEDREIRA, ANTONIO S. *Bibliografía puertorrigueña (1493–1930).* (Univ. de Puerto Rico, Monografías, ser. A; Estudios Hispánicos, *1.*) Madrid: Hernando, 1932. 709 p. DLC—Z1551.P37

13,000 ±. Subject, topic, author. Indexes: author and subject. Includes section "Antropología y etnología," p. 63–85. [216

RAGATZ, LOWELL J. *A check-list of House of Commons sessional papers relating to the British West Indies and the West Indian slave trade and slavery, 1763–1834.* 2d ed. London: Bryan Edwards, 1928. 32 p. DLC—Z1502.B5R2 1928

500 ±. Bills, parliamentary reports, accounts, papers and subject. [217

RAGATZ, LOWELL J. *A check-list of House of Lords sessional papers relating to the British West Indies and to the West Indian slave trade and slavery, 1763–1834.* London: Bryan Edwards, 1931. 13 p. 2d ed. 1932, 16 p. DLC—Z1502.B5R21

140 ±. Subject (bills, reports, accounts and papers), topic, chron. About 50 items relate to slavery. [218

REID, CHARLES F. (ed.) . *Bibliography of the Virgin Islands of the United States. . . .* New York: Wilson, 1941. 225 p. DLC—Z1561.V8R4

4,000 ±. Subject, author. Index: author and subject. Brief annotation. Anthropology, 22 items; archaeology, 14 items; sociology, 23 items. [219

ROTH, H. LING. "Bibliography and cartography of Hispaniola," *Supplementary Papers of the Royal Geographic Society*, **2**, part 1, n.d.: 41–97. DLC—G7.R81 vol. 2; Z1531.R84

530 ± plus 120 ± maps. Kind of publication (named author; anonymous: parliamentary publications; maps, charts, and plans), author or chron. Some items annotated. Index; subject ("aboriginees," 28; "agriculture," 4; "people and populations," 9, etc.). Chronological table of authors. [220

STEWARD, JULIAN H. (ed.). *Handbook of South American Indians; vol. 4, the circum-Caribbean tribes.* (U. S. Bureau of American Ethnology, Bulletin, *143*, **4**.) Washington, D.C.: Government Printing Office, 1948. References at ends of articles, combined bib. at end of volume. DLC—E51.U6 no. 143, vol. 4; F2229.S75 no. 143 vol. 4

500 ±. Author. [221

1.23 Western South America

ANGELES CABALLERO, CÉSAR A. *Bibliografía del folklore peruano.* Lima: 1952. 23 p. ("Primera Contribución.") DLC—Z5984.P4A8

200 ±. Author. Some items briefly annotated. [222

BARRERA B., JAIME. "Bibliografía para el estudio de la prehistoria ecuatoriana," *Anales de la Universidad Central* (Quito), **58**, *299*, 1937: 99–149. DLC—AS83.Q8 vol. 58

200 ±. Chronological period. Annotated. Concerns chiefly ethnohistorical sources for the study of Inca culture. [223

BASADRE, JORGE. "Bibliografía de la literatura quechua," *Literatura inca.* (Biblioteca de Cultura Peruana, Primera Serie, 1.) Paris: Desclee de Brouwer, 1938: 19–23. DLC—PQ8433.B5 vol. 1

80. A: ollanta texts, B: other texts, C: commentaries on ollanta, D: other commentaries on Quechua literature in general. A, B, and C chron., D by author. [224

BRAND, DONALD D. "A brief history of Araucanian studies," *New Mexico Anthropologist* (Albuquerque) , **5**, *2*, 1941. Bib.: *passim*, 19–35. DLC—E77.N45 vol. 5

130 ±. Subject, author (except ethnography 1824–91 chron.) Some items annotated. Most important works starred. [225

BRAND, DONALD D. "The peoples and languages of Chile," *New Mexico Anthropologist* (Albuquerque) , **5**, *3*, 1941. Bib.: 86–99. DLC—E77.N45 vol. 5

180 ±. Author. Supplements the bibliography in Brand's "The status of anthropology in Chile," and his "A brief history of Araucanian studies," in the same journal. [226

BRAND, DONALD D. "The status of anthropology in Chile," *New Mexico Anthropologist* (Albuquerque) , **5**, *3*, 1941. Bib.: *passim*, 55–71. DLC—E77.N45 vol. 5

60 ±. Subject, author. [227

CASTELLVÍ, MARCELINO DE. "Bibliografía del idioma Kamsá (o Coche) de Sibundoy (Putumayo-Colombia)," *Proceedings of the Third Convention of the Inter-American Bibliographical and Library Association, 1940.* (Publications, Ser. 2, **3**.) Washington, D.C.: 1941: 96–97. DLC—Z1008.I578 vol. 3

18. Author. Annotated. [228

CASTELLVÍ, MARCELINO DE. "Bibliografía sobre los Sibundoyes y otros de la familia lingüística Kamsá o Coche," *Proceedings of the Third Convention of the Inter-American Bibliographical and Library Association, 1940.* (Publications, Ser. 2, **3**.) Washington, D.C.: 1941: 86–95. DLC—Z1008.I578 vol. 3

90 ±. Kind of work (published, unpublished, basic and complementary), author. Annotated. [229

CASTELLVÍ, MARCELINO DE. "Las investigaciones lingüísticas y etnográficas en la Misión del Caquetá," *Boletín de Estudios Históricos* (Pasto, Colombia), **5**, *55*, 1935. Bib.: 199–213. DLC—F2251.B63 vol. 5

170. Linguistics and ethnology, tribe. Some items annotated. [230

DORSEY, GEORGE A. *A bibliography of the anthropology of Peru.* (Field Columbian Museum, Anthropological Series, **2**, *2*: 55–206; Publication, *23*.) Chicago: 1898. DLC—GN2.F4 vol. 2 no. 2; Z5114.D71

1,500 ±. Author. A few items annotated, some with biographical sketches of the authors. [231

HORKHEIMER, HANS. "Breve bibliografía sobre el Perú prehispánico," *Fénix, Revista de la Biblioteca Nacional* (Lima) , **5**, 1947: 209–82. Bib.: 234–82. DLC—Z761.F35 vol. 5

360 ±. Author. Preceding text provides annotation. [232

LARREA, CARLOS M. *Bibliografía científica del Ecuador.* (Editiones Cultura Hispanica.) Madrid: 1952. "Parta cuarta: anthropología, etnografía, arqueología, lingüística, folklore, prehistoria, historia antigua," p. 193–275, and supplement, p. 476–80. DLC—Z7407.E25.L29

1,598. Author. [233

LATCHAM, RICARDO E. "Bibliografía chilena de las ciencias antropológicas," *Revista de Bibliografía Chilena y Extranjera* (Santiago) , **3**, *6*, *7*, 1915: 148–85, 229–61. DLC—Z1701.R4 vol. 3

1,172. Author. An expansion of CARLOS E. PORTER, "Bibliografía chilena de antropología y etnología." Includes the 80 items listed in RICARDO E. LATCHAM, "Bibliografía chilena de antropologiá y etnología (1909–1913)," *Revista de Bibliografía Chilena y Extranjera*, **2**, *1–2*, 1914: 49–52. [234

MEANS, PHILIP A. *Biblioteca andina. Part one. The chroniclers . . . of the sixteenth and seventeenth centuries who treated of the prehispanic history and culture of the Andean countries.* (Connecticut Academy of Arts and Sciences, Transactions, *29:* 271–525.) New Haven: 1928. DLC—Q11.C9 vol. 29

250 ±. Author. Annotated. Illustrations of title pages. Biography and discussion of work done by each author. List of sources in the introduction. [235

MEANS, PHILIP A. *A study of Peruvian textiles. . .* Boston: Museum of Fine Arts, 1932. Bib.: 74–82. DLC—NK8839.M4

120 ±. Author. [236

MEDINA, JOSÉ T. *Bibliografía de las lenguas quechua y aymará.* (M. A. I., H. F., Contributions, **7**. *7*.) New York: 1930. 117 p. DLC—E51.N42 vol. 7

203. Dated publications, undated publications, and manuscripts; chron. Index: author. Annotated. Facsimiles of title pages of some early items. [237

MUELLE, JORGE C. "Bibliografía antropológica del Perú 1943–45," *B. B. A. A.*, **8**, 1945: 48–64. DLC—Z5112.P3 vol. 8

300 ±. Subject and author. [238

NAVARRO DEL AGUILA, VÍCTOR. "Contribución a la bibliografía del folklore peruano," *Revista del Instituto Americano de Arte* (Cuzco, Peru) , **1**, Año 4, 1945: 33–52. DLC—AP63.I585

120 ±. Subject, author. Annotated. [239

ORTIZ, SERGIO E. "Contribución a la bibliografía sobre ciencias etnológicas de Colombia," *Idearium* (Organo de la Escuela Normal de Occidente), Suplemento *1*. Pasto, Colombia: 1937. 66 p. Bib.: 17–66. DLC—Z5114.E42

520 ±. Subject, author. Includes physical anthropology, linguistics (preliminary), ethnography, and general anthropology. [240

PEREIRA SALAS, EUGENIO. *Guía bibliográfica para el estudio del folklore chileno.* (Univ. de Chile, Instituto de Investigaciones Folklóricas 'Ramón A. Laval,' Archivos del Folklore Chileno, *4*.) Santiago de Chile: 1952? 112 p. DLC—GR133.C5A7 no. 4

1,289. Subject, topic, author. Items 1,078–1,271 concern Chilean aborigines, especially the Araucanians. [241

PORTER, CARLOS E. "Bibliografía chilena de antropología y etnología," *Trabajos del Cuarto Congresso Científico (1° Pan-Americano)* [1908–09], **14** (Sec. III: Ciencias Naturales, Antropológicas y Etnológicas; Trabajos, **2**). Santiago: Balcells, 1911: 109–36. DLC—AS4.P2 vol. 2

——— ——— Reprint, *Anales del Museo Nacional de Buenos Aires*, Ser. 3, **13**, 1911: 147–88. DLC—Q33.B8 ser. 3, t. 13.

——— ——— Reprint, *Boletín del Museo Nacional de Chile* (Santiago), **3**, *2*, 1911: 401–41. DGS—S (430) M97 vol. 3

——— Reprint. *Bibliografía chilena de ciencias antropológicas*. Santiago de Chile: Santiago, 1912. 62 p. DSI—Ethnology

200 ±. Author. Annotated, some items extensively. Supplemented by RICARDO E. LATCHAM, "Bibliografía chilena . . ." *q. v.* Supersedes CARLOS E. PORTER, "Literatura antropolójica i etnolójica de Chile," Santiago: Soc. Imprenta i Litografia Universo, 1906. 36 p. (Extract from *Revista Chilena de Historia Natural* [Valparaíso] **10**, *2*, 1906: 101–27.) [242

PORTER, CARLOS E. "Les études antropologiques au Chili," *Jour. de la Société des Américanistes de Paris*, n.s. **7**, *1–2*, 1910: 203–19. DLC—E51.S68 vol. 7

200 ±. Author. Identical with Porter's 1911 bibliography except annotation lacking. [243

REICHEL-DOLMATOFF, GERARDO. *Bibliografía etnológica del Departamento del Magdalena*. (Instituto Etnologico del Magdalena, Divulgación Cultural *3*.) Santa Marta, Colombia: 1949, 27 p.

200 ±. Subject, author. [244

REICHEL-DOLMATOFF, GERARDO. "Bibliografía lingüística del grupo chocó," *Boletín de Arqueología* (Bogotá), **1**, *6*, 1945: 625–27. DLC—F2269.B7 vol. 1

40 ±. Author. [245

RICHARDSON, FRANCIS B., and ALFRED KIDDER, II. "Publicaciónes estadunidenses y británicas sobre la arqueología peruana," *Boletín Bibliográfico de la Univ. de San Marcos* (Lima), **10**, 1940: 13–19d. DLC—Z782.L77B vol. 10

140 ±. Subject, author. Works published 1935–40. Important items starred. [246

RIVET, PAUL, and GEORGES DE CRÉQUI-MONTFORT. *Bibliographie des langues aymará et kičua*. (Univ. de Paris, Institut d'Ethnologie, Travaux et Mémoires, *51*.) Paris: 1951. 4 vol. 2,895 p. DLC—Z7122.A9R5

5,800 ±. Chron.(vol. 1: 1540–1875; vol. 2: 1876–1915; vol. 3: 1916–1940; vol. 4: 1941–1955), author. Annotated; facsimiles of title pages of many items. Indexes: persons and religious bodies; periodicals, series, and honorary collections; places, tribes, and peoples; languages and dialects; subject. Each volume contains an appendix of corrections and additions. [247

SAVILLE, MARSHALL H. *Bibliographic notes on the shrinking of human heads in South America*. (M. A. I., H. F., Indian Notes, **6**, *1:* 56–74.) New York: 1929. DLC—E51.N44 vol. 6

65 ±. Chron. Annotated, including discussions of the subject. [248

SAVILLE, MARSHALL H. "Bibliography of the anthropology of Ecuador," *Contributions to South American archeology: The George G. Heye expedition* (New York: 1907), **1**: 121–35. DLC—F3721.1.M2S3 vol. 1

165. Original and supplementary lists, author. [249

SCHWAB, FEDERICO. "Bibliografía de antropología peruana 1936–37," *Boletín Bibliográfico de la Univ. de San Marcos* (Lima), **8**, Año 11, *1*, 1938: 48–85. DLC—Z782.L77B vol. 8

167. Subject, author. Index: author. Annotated. [250

SCHWAB, FEDERICO. "Bibliografía de etnología peruana," *Boletín Bibliográfico de la Univ. de San Marcos* (Lima), **6**, Año 9, 1936: *1:* 1–26; *2:* 4–27; *3–4:* 101–14. DLC—Z782.L77B vol. 6

1,500 ±. Subject, author. [251

SCHWAB, FEDERICO. "Bibliografía etnológica de la Amazonía peruana," *Boletín Bibliográfico de la Univ. de San Marcos* (Lima), **12**, Año 15, *3–4*, 1942: 205–71. DLC—Z782.L77B vol. 12

——— Reprint with minor changes: *Bibliografía etnológica de la Amazonia peruana*, 1542–1942. Lima: Compañia de impresiones y publicidad, 1942. 76 p. DLC—Z5114.838

400 ±. Author. Annotated: gives locations. [252

SUNDT, ROBERTO. "Bibliografía araucana," *Revista de Bibliografía Chilena y Extranjera* (Santiago), **5**, 1917: 300–15; **6**, 1918: 3–20, 87–101, 182–213, 269–86, 345–60, 401–18. DLC—Z1701.R4

717. Author. Annotated. Completed only for authors whose names begin with letters A through L. [253

TAURO, ALBERTO. "Bibliografía indianista peruana," *Boletín Bibliográfico de la Univ. de San Marcos* (Lima), **16**, *3–4*, 1946: 283–308. DLC—Z782.L77B vol. 16

155. Subject, topic, author. Index: author. Annotated. [254

TELLO, JULIO C. "Bibliografía antropológica del Perú," *Boletín Bibliográfica de la Univ. mayor de San Marcos* (Lima), **3**, 1927: 31–36. DLC—Z782.L77B vol. 3

100 ±. Limited to books in the archives of the museum which have not been cited in earlier bibliographies. [255

TRUJILLO FERRARI, ALFONSO. "As ciências sociais no Perú nos últimos quinze anos," *Sociologia* (São Paulo), **16**, *2*, 1954: 36–106. DLC—HM7.S57 vol. 16

422. Subject (physical anthropology and archaeology, human ecology and geography, ethnology and ethnography, folklore, linguistics, and sociology), topic, author. Index: author. Some items annotated. [256

UHLE, MAX. "Bibliografía ampliada sobre etnología y arqueología en la Ecuador," *Anales de la Universidad Central* (Quito), **43**, *270*, 1929: 453–89. NcD—378.86Q8 no. 269–270.

384. Chron. 1534–1929. Few items annotated. Earlier versions appeared in the same journal as follows: **37**, *257*, 1926: 166–77. OU—AS83.E19 no. 37–39. 168 items. **38**, *259*, 1927: 234–35. OU—AS83.E19 no. 37–39. 19 item supplement. **42**, *267*, 1929: 53–83. CU—LE46.Q5A3 v. 42. 338 item revision. [257

1.24 Eastern South America

ANDRADE, MARIO DE. "Foclore," in RUBENS BORBA DE MORAES and WILLIAM BERRIEN, *Manual bibliográfico de estudos brasileiros*. Rio de Janeiro: Souza, 1949. Bib.: 299–317. DLC—Z1686.B6

178. Author. Annotated. [258

ANGYONE COSTA, JOÃO. *Introdução á arqueologia brasileira, etnografia e historia*. 2d ed. (Biblioteca Pedagogica Brasileira, Ser. 5; Brasiliana, **34**.) São Paulo: Companhia editora nacional, 1938. Bib.: 326–38. DLC—F2519.A65

160 ±. Author. [259

ARANA, ENRIQUE, JR. "Bibliografía de lenguas americanas," *Boletín del Instituto de Investigaciones Históricas* (Buenos Aires: Univ. Nacional), año 10, **13**, *49–50*, 1931: 138–48. DLC—F2801.B96 vol. 13; Z7122.A3A6

100 ±. General works by author, vernacular works by language; chron. Annotated. Constitutes an addition to JOSÉ T. MEDINA, *Bibliografía de la lengua guaraní*. [260

AYROSA, PLINIO. *Apontamentos para a bibliografia da lingua tupi-guarani*. (Univ. do São Paulo; Faculdade de Filosofia, Ciências e Letras; Boletim, **33**; Etnografia e língua tupí-guaraní, *4*.) São Paulo: 1943. 303 p. DLC—PM7171.A35 no. 4

585. Author. Index: author, region, tribe, serial, etc. Annotated. Facsimiles of some frontispieces. [261

BALDUS, HERBERT. *Bibliografia comentada da etnologia brasileira, 1943–1950*. (Série Bibliográfica de Estudos Brasileiros, *1*.) Rio de Janeiro: Souza, 1954. 142 p. DLC—Z5114.B28

354. Author. Annotated. Lists of journals and monograph series appended. Supplements his "Etnologia" in RUBENS BORBA DE MORAES and WILLIAM BERRIEN, *Manual bibliográfico de estudos brasileiros*, 1949. [262

BALDUS, HERBERT. *Bibliografia crítica da etnologia brasileira*. (Commissão do IV Centenário da Cidade de São Paulo.) São Paulo: Serviço de Comemorações Culturais, 1954. 859 p. DLC—Z5114.B3

1,785. Author. Indexes: subject, geographical, tribal, and author. Annotated. [263

BALDUS, HERBERT. "Etnologia," in RUBENS BORBA DE MORAES and WILLIAM BERRIEN, *Manual bibliográfico de estudos brasileiros*. Rio de Janeiro: Souza, 1949. Bib.: 209–55. DLC—Z1686.B6

254. Author. Author index. Annotated. [264

BARROS PAIVA, TANCREDO DE. "Bibliographia ethnica-linguistica brasiliana," *Annaes do XX Congresso Internacional de Americanistas*, 1922, **3**: 331–400. Rio de Janeiro: 1932. DLC—E51.I7, vol. 3

600 ±. Initial list and supplement, subject, author. Some items annotated. Includes sections on linguistics, Negroes and Mestizos, and folklore. [265

BARROS PAIVA, TANCREDO DE. "Raça brasílica: usos, costumes, linguagem; ethnografia e antropologia. Bibliografia," *Boletim do Ministerio da Agricultura, Industria, e Commercio* (Rio de Janeiro), Anno 19, **1**, *1* and *2*, 1930: 121–40, 255–71. DLC—HC186.A3 vol. 19

728 in the two issues listed. Subject (I—"Catechese e historia," II—"Ethnographia e anthropologia"), author. Some items annotated. Sec. II completed for authors A- through Maximiliano only. Continuation not published. [266

"Bibliografía antropológica argentina," *Anuario de Historia Argentina* (Buenos Aires: Sociedad de Historia Argentina), 1–2, 1939–40. Annual. DLC—F2801.A63; (1940 only) Z5114.C25

110–150 ±. Author. Compiled by Julián B. Cáceres Freyre. [267

"Bibliografía antropológica argentina . . . ," *B. B. A. A.*, 3–5, 1939–41; 7–8, 1943–45; 11, 1948. DLC—Z5112.P3

50–150 ±. Author. Covers publications appearing 1938–44 and 1946–47. Compilers: Julián B. Cáceres Freyre, 1939–41; Osvaldo L. Paulotti, 1943–44; Benigno Martínez Soler, 1948. [268

BRAND, DONALD D. "A brief history of anthropology in Brasil," *New Mexico Anthropologist* (Albuquerque), 5, *4*, 1941. Bib.: 124–50. DLC—E77.N45 vol. 5

420 ±. Includes general works, explorations, time periods, regional histories, religious accounts, etc. Lists several editions and translations for some items. [269

CARNEIRO, EDISON. *O folclore nacional, 1943–1953.* (Série Bibliográfica de Estudos Brasileiros, *2.*) Rio de Janeiro: Souza, 1954. 73 p. DLC—Z5984.B7C3

205. Author. Annotated. List of periodicals analyzed. [270

CARVALHO NETO, PAULO DE. "Bases bibliográficas para el estudio sistemático de la antropología paraguaya," *B. B. A. A.*, 13, *1*, 1951: 179–210. Bib.: 188–210. DLC—Z5112.P3 vol. 13

430. Culture group and folklore, chron. [271

CASTELLVÍ, MARCELINO DE. "Bibliografía de la familia lingüística piaroa o saliba," *Inter-American Bibliographical Review*, 2, 1942–43: 238–42. DLC—Z1008.I583 vol. 2

31. Language, author. Brief annotation. Concerns a language group in southern Venezuela. [272

CASTELLVÍ, MARCELINO DE. "Bibliografía sobre la familia lingüística tukano (antes betova)," *Proceedings of the Second Convention of the Inter-American Bibliographical and Library Association, 1939.* (Publications, Ser. 2, 2.) New York: Wilson, 1939: 97–105. DLC—Z1008.I578 vol. 2

62. Tribe and author. Concerns a group in western Brazil. Koch Grünberg's classification of the Tukano language family reproduced. [273

COOPER, JOHN M. *Analytical and critical bibliography of the tribes of Tierra del Fuego and adjacent territory.* (U. S. Bureau of American Ethnology, Bulletin, *63.*) Washington, D. C.: Government Printing Office, 1917. 233 p. DLC—E51.U6 no. 63; Z1210.F9C7

750 ±. Author. Indexes: subject and tribe. Annotated. [274

CORTÁZAR, AUGUSTO R. *Guía bibliográfica del folklore argentino.* (Univ. de Buenos Aires, Instituto de Literatura Argentina, Sección de Bibliografía, 1, *1.*) Buenos Aires: 1942. 293 p. DLC—Z5984.A7.C6

867. Subject. Index: author. Sparsely annotated. Locations of items given. [275

ERNST, ADOLFO. "Ensayo de una bibliografía de la Guajira y de los Guajiros," *Revista Científica de la Univ. Central de Venezuela* (Caracas), 2, *20*, 1890: 341–57. PPAN—QH7.V4 vol. 2

106. Items seen and items not seen by compiler, chron. Annotated. [276

FARIA, L. DE CASTRO. *Pesquisas de antropologia física no Brasil.* (Rio de Janeiro, Museu Nacional, Boletim, n.s., Antropologia, *13.*) 1952. Bib.: 63–106. DPU

430 ±. Author. Index: subject. Preceding text contains summaries and comments on the items. [277

GORHAM, REX. *The folkways of Brazil, a bibliography.* KARL BROWN (ed.). New York: Public Library, 1944. 67 p. DLC—Z5984.B7G6

344. Subject. Indexes: author, title, subject. Annotated. Glossary. Excludes Amazonian folklore. [278

HISS, PHILIP H. *A selective guide to the English literature on the Netherlands West Indies. With a supplement on British Guiana.* (Netherlands Information Bureau, Booklets *9.*) New York: 1943. Supplement p. 114–24. DLC—Z1502.D7H5

100 ± items in supplement (19 listed under "anthropology"). Subject, author. Index: author. [279

LÓPEZ RAMÍREZ, TULIO. "Materiales para la bibliografía de antropología física venezolana," *Acta Venezolana* (Caracas). 1, *4*, 1946: 445–58. DLC—F2301.A25 vol. 1

100 ±. Author. Annotated. A preliminary list. [280

LOUKOTKA, ČESTMIR. *Les langues de la famille tupi-guarani.* (Univ. de São Paulo; Faculdade de Filosofia, Ciências e Letras; Boletim, 104; "Etnografia e Lingua Tupi-guarani," *16.*) São Paulo: 1950. 42 p. Bib.: 31–40. DLC—PM7171. A35 no. 16

150 ±. Author. Bibliography preceded by a list of tribes and languages, with references to the bibliography. [281

LOUKOTKA, CHESTMÍR. "Linguas indígenas do Brasil-distribuição e bibliografia," *Revista do Archivo Municipal* (São Paulo: Dept. de Cultura e de Recreação), *54*, 1939: 147–74. Bib.: 165–74. CU—F2501. S3 1939

200 ±. Author. Bibliography preceded by catalog of languages, with references to the bibliography. [282

MEDINA, JOSÉ T. *Bibliografía de la lengua guaraní.* (Buenos Aires, Universidad Nacional, Instituto de Investigaciones Históricas, Publicaciones, *51.*) Buenos Aires: Peuser, 1930. Bib.: 23–93. DLC—Z7122.G9M4

144. Chron. Index: author. Annotated, early items extensively. [283

PIERSON, DONALD. "Racial and cultural contacts in Brazil: present state of research in this field," *H. L. A. S. 6*, 1940: 463–70, *passim*. DLC—Z1605.H23 no. 6

108. Text provides annotation. [284

PIERSON, DONALD, and MÁRIO WAGNER VIEIRA DA CUNHA. "Pesquisa e possibilidades de pesquisa no Brasil," *Sociologia* (São Paulo), 9, *3–4*, 1947: 234–56, 350–78. DLC—HM7.S57 vol. 9

—— English version. "Research and research possibilities in Brasil with particular reference to culture and cultural change," *Acta Americana*, 5, *1–2*, 1947: 18–82. DLC—E51.A3 vol. 5

120 ±. Bibliographies and recent works, author. [285

POURCHET, MARIA JULIA. *Índice cefálico no Brasil: Revisão Bibliográfica.* Rio de Janeiro: 1941. Bib.: 51–58. DLC—GN93. B7P6

80 ±. Author. [286

RAMOS, ARTHUR. "Os estudos antropológicos e sociológicos no Brasil," *H. L. A. S.* 1936: 372–86. DLC—Z1605.H23 1936

230 ±. Subject, author. [287

SCHADEN, FRANCISCO S. G. "Apontamentos bibliográficos para o estudo dos índios kaingang," *Boletim Bibliográfico* (São Paulo: Biblioteca Pública Municipal), ano *1*, 2, 1944: 23–32. DLC—Z907.S3 vol. 2

80. Author. Concerns a group in southeastern Brazil. [288

SCHADEN, FRANCISCO S. G. "Apontamentos bibliográficos para o estudo dos índios xokléng," *Boletim Bibliográfico* (São Paulo: Biblioteca Pública Municipal), 12, 1949: 113–19. DLC—Z907.S3 vol. 12

46. Author. Concerns a group in southern Brazil. [289

VALLE CABRAL, ALFREDO DO. "Bibliographía das obras tanto impressas como manuscriptas relativas á lingua tupi ou guarani, tambem chamada lingua geral do Brazil," *Bibliotheca Nacional do Rio de Janeiro, Annaes*, 8, 1880–81: 143–214. DLC—Z1675.R58 vol. 8

302. Kind of work (works dealing primarily with the language, works which include some linguistic materials, manuscripts), topic, chron. Index: author. Annotated, some items extensively. [290

42, 43, 44, 50, 67, 68, 69, 70, 71, 72, 80, 82, 85, 91, 101, 102, 124, 125, 148, 159, 160, 167, 169, 171, 174, 181, 185, 187, 188, 190, 198, 199, 203, 205, 211, 226, 228, 230, 233, 234, 237, 245, 247, 260, 261, 265, 272, 273, 281, 282, 283, 290. *See also* Vernacular works
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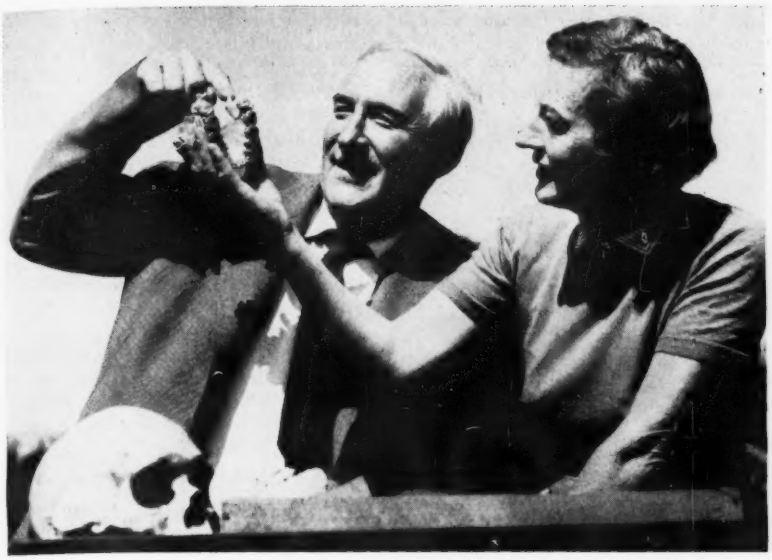

Louis and Mary Leakey examining the palate of *Zinjanthropus boisei*

a general resemblance to the paranthropine type of australopithecine population in southern Africa, is distinguished by a number of features paralleled only in hominid populations of the Middle Pleistocene from other areas of the Old World.

3) Much has been written of the food-getting behavior of the southern African australopithecines. A number of workers, including the writer, have concluded that these primitive hominids were carnivorous and predaceous, augmenting their diet with smaller mammals, although incapable of dealing with the larger herbivores and other types of mammals of the Villafranchian. Such behavior would necessitate some sharp-edged stone implements for skinning and dismembering prey since the anterior teeth of australopithecines were small and of essentially human form. The discovery of the FLK site clearly indicates that such creatures were indeed predators of small animals, including small mammals and immature larger mammals, and also birds. This is a momentous discovery bearing directly on the origin and evolution of human behavior.

4) The lower beds (I, II) of the Olduvai series were long regarded as representing the earlier segment of the Middle Pleistocene. However, it is now clear from the Leakeys' extensive work in these beds that Bed I, both on stratigraphic and especially faunal grounds, is more distinct from Bed II than was previously believed. Bed I equates in age broadly with the richly fossiliferous Omo beds (north of Lake Rudolph) which are referred to the (upper) Villafranchian and, hence, the later Lower Pleistocene. (These beds recently provided the writer a small series of Pre-Chelles-Acheul implements, the first discovered there.) Since hominid occupation is unknown in Europe or Asia before the earlier Middle Pleistocene (cf. Howell, *Science,* in press) the new australopithecine from Olduvai Gorge represents the oldest, fully authenticated, toolmaker so far known.

The Leakeys are to be warmly congratulated for their perseverance in providing, in the words of LeGros Clark, another important part of "the crucial evidence for human evolution."

* "Zinj" was the Arabic name applied in classical times to eastern Africa. The Boise Fund has long supported research at Olduvai Gorge.

Meetings

▶ Any Associate who has not received the preliminary announcement for the SIXTH INTERNATIONAL CONGRESS OF ANTHROPOLOGICAL AND ETHNOLOGICAL SCIENCES should write for a copy to: Prof. H. V. Vallois, Director, Musée de l'Homme, Palais de Chaillot, 16e, Paris, France. A second announcement will be issued in February, 1960. The Congress will be in Paris, July 30–August 6, 1960.

Meetings of the Congress in past years were held in London, 1934, Copenhagen, 1938, Brussels, 1948, Vienna, 1952, and Philadelphia, 1956. The Sixth Congress will have three sections of papers; anthropology (physical), ethnology (social and cultural topics), and museology. The papers will be published after the Congress, and distributed to the participants who pay the 5,000 Franc ($10.00) registration fee at the Congress.

34th International Congress of Americanists

Vienna, July 18–25, 1960

▶ Since 1875, the Congress of Americanists has met every two years, alternating between Europe and America. Papers deal with all branches of our study, broadly conceived, so far as they concern America. Subscription is $10.00 U.S., or 256.00 Austrian schillings for full members, who will be entitled to vote and to receive the Proceedings of the Congress when they are published. Fees should be sent in advance; if this cannot be arranged, they may be paid on arrival, provided an application for membership has been submitted.

Circular No. 1, describing the Congress and containing a membership application form, is available from Dr. Robert Heine-Geldern, President of the Organizing Committee, Institute für Völkerkunde, Reitschulgasse 2, Vienna I, Austria.

Calendar

1959

December 26–31. American Association for the Advancement of Science. Chicago, Ill. Morrison, Hamilton, LaSalle, and Sherman Hotels, Exec. Officer, A.A.A.S., Dael Wolfle, 1515 Massachusetts Avenue, N.W., Washington, 5, D. C.

December 27–30. American Anthropological Association. 58th Annual. Mexico City. Majestic and Ritz Hotels, Program Chrmn., Dr. Ignacio Bernal, Tres Picos No. 65, Mexico D.F., 5.

1960

July 18–25. 34th International Congress of Americanists. Vienna, Austria. Information: Prof. Robert Heine-Geldern, Institut für Völkerkunde, Reitschulgasse 2, Vienna I, Austria.

July 30–August 6. 6th International Congress of Anthropological and Ethnological Sciences. Paris, France. Chrmn., Prof. H. V. Vallois, Director, Musée de l'Homme, Palais de Chaillot 16e, Paris, France.

August 1–5. International Conference on General Semantics. Honolulu, Hawaii, U.S.A. Gen. Chrmn., Dr. Shunzo Sakamaki, Prof. of History, University of Hawaii, Honolulu, Hawaii, U.S.A.

August 10–17. International Congress of Orientalists. Moscow, U.S.S.R.

November 17–20. American Anthropological Association. 59th Annual. Minneapolis, Minnesota. Program Chrmn., Dr. Robert Spencer, Dept. of Anthropology, University of Minnesota, Minneapolis, Minn., U.S.A.

1961

Late August–September. 10th Pacific Science Congress. Honolulu, Hawaii, U.S.A. Information: Robert W. Hiatt, Univ. of Hawaii, Honolulu, Hawaii, U.S.A.

Meetings of broad interest will be announced as long in advance as known.

THE NEWEST LINK IN HUMAN EVOLUTION:

The Discovery by L.S.B. Leakey of *Zinjanthropus boisei*

IT IS FORTUNATE that this first issue can include a news story of the kind that can "break" only rarely. It also has a personal meaning for the Editor. On July 7th the Editor spent some hours with L. S. B. Leakey in Nairobi. Leakey showed off a large hominid tooth he had found in Bed I at Olduvai Gorge. Leakey had planned to accompany Clark Howell to the Omo beds in Southern Ethiopia in the following week, but now was so anxious to return to the site in Tanganyika that he cancelled this plan. He seemed sure that he was on the verge of great discoveries. He was.

A month later he startled the Pan African Congress on Prehistory at Leopoldville with a first unveiling.

There follows here, first, Leakey's personal, "very preliminary" account, and then a note by Clark Howell pointing to the significance of the find. Howell first visited Olduvai in 1954, excavated Isimila, an Acheulean occupation site in southern Tanganyika, and this summer made a reconnaisance to the Villefranchian Omo beds. He was the first anthropologist to whom Leakey revealed his new discovery.

Leakey's Account:

On July 17th this year (1959), Mrs. Leakey and I were again at work at Olduvai Gorge in Tanganyika Territory. The site being searched was FLK I, an area of erosion gullies cutting into Bed I and Bed II of the Olduvai sequence. This site had yielded me the very first evidence of the Oldowan culture, during my 1931–32 season, but since then it had not been worked in any detail because there were so many other parts of the gorge that remained to be studied.

My wife was working her way slowly up the exposures, when at about 11 A.M. she saw a small fragment of fossil skull, part of the temporal, on the scree, washed out by erosion. Following up this clue, she saw protruding from the cliff three hominid teeth. She at once called me, and it was apparent that a most unusual discovery was before us.

The excavation, which continued until August 6th, resulted in uncovering a part of living floor of the Oldowan culture, at a depth of about 22 feet below the top of Bed I.

This floor yielded the nearly complete skull of an australopithecine, stone tools of the culture and the fossil remains of some rodents, reptiles, birds and immature large mammals which had formed part of the diet of the makers of the Oldowan culture at this spot.

Here, for the first time, we had a representative fossil of the actual maker of the Oldowan culture, found on his living-site. Here was the oldest yet known, stone tool maker.

After preliminary study, I have put the new find into a new genus of the australopithecinae, which I have called *Zinjanthropus*, with the specific name of *boisei*.

The new skull shows some features which resemble those seen in *Paranthropus*, others which can be seen in *Australopithecus*, and many which occur in neither and are closer to *Homo*. In particular, the architecture of the malar-maxillary region, the mastoids, the frontal sinus, as well as the reduction of the upper third molars are of a special interest.

Resemblances to *Paranthropus* include a small sagittal crest, but there is also a nuchal crest which is not, however, connected with the sagittal one.

The over-all size of the teeth is very great. I am not yet willing to estimate the cranial capacity and the estimate given to the popular press, and alleged to have been made by me, was never given by me at all.

The palaeontological and geological evidence places this skull in the upper half of the Villafranchian, and generally contemporary with the Taungs *Australopithecus* child, and the *Australopithecus* specimens from the lower deposits at Sterkfontein.

The Makapan, Swartkrans, Upper Sterkfontein and Kromdraai australopithecines are all younger. Similarly, on the faunal evidence, the Djetis beds of Java now seem to fall in the lower half of the Middle Pleistocene, so that the new find in the upper part of the Lower Pleistocene is also older than any *Pithecanthropus*.

Howell's Comments:

The Leakeys have made a discovery of extraordinary importance for human palaeontology. Its significance is severalfold, as is briefly indicated here. I am deeply grateful to them for permission to examine the specimens prior to the public announcement of their discovery.

1) The makers of the Oldowan implements, the first Pre-Chelles-Acheul industry (often termed "pebble-tools") ever recorded *in situ*, have always been an enigma. From this new unique discovery of a hominid occupation surface at the FLK open-air site at Olduvai Gorge it is clear that an australopithecine group was responsible. The question of the implement-making capabilities of the australopithecines has long been debated and the discovery of Pre-Chelles-Acheul implements at Sterkfontein a few years ago seemed to go a long way toward resolving the question. However, Robinson has recently indicated that the implement-bearing breccia at Sterkfontein, which also contains bones of *Equus*, is stratigraphically distinct, and clearly later, than the main australopithecine bearing breccia at that site. Moreover, further recent work at Sterkfontein has provided a more extensive collection of stone artifacts, including some crude early Chellean-type bifaces, suggesting that the assemblage in question may really represent an early stage of Chelles-Acheul industry, like that now well known from the very base of Bed II (site BK II) at Olduvai Gorge. Milk teeth of a primitive hominid are known from the latter site and show a number of australopithicine features; perhaps the fragmentary maxilla and teeth from the implement-bearing breccia at Sterkfontein, which are australopithecine in morphology, represent the same creature?

2) This new hominid from the FLK site is clearly an australopithecine though it differs in certain aspects of cranial morphology from any of the forms known previously. If the differences between the South African forms (*Australopithecus* and *Paranthropus*) are regarded as justifying generic rank then generic status is equally warranted for the East African form which Leakey has now designated *Zinjanthropus boisei*.* It is of particular interest that this australopithecine, which bears

Institutions

▶ THE FAR-EASTERN PREHISTORY ASSOCIATION was founded as an offshoot of the Pacific Science Congress, and held three meetings before World War II. In 1953, at a fourth meeting, held in Manila conjointly with the Eighth Pacific Science Congress, the Association was more formally organized and began the formation of Branches in member countries or areas. In 1957 the newly formed American Branch began publication of a biannual bulletin, called *Asian Perspectives,* which is now sponsored by the Association as a whole. The pre-history of Oceania and Asia (exclusive of India) is its main subject matter, but it is planned to include the whole field of archaeology, as well as all other subdisciplines of anthropology having any bearing on archaeology. Each summer issue will contain news of fieldwork and research and bibliographic information, with some short articles. The winter issues will have requested papers on a central theme. The winter number for 1958 (expected to appear in December, 1959) will be on the Far Eastern palaeolithic, with articles on Japan, China, Philippines, Borneo, Celebes, Thailand, and Malaya. Hallam L. Movius, Jr., Peabody Museum, Harvard University, is guest editor.

Asian Perspectives is now published by the Hong Kong University Press. Membership dues and subscriptions for 1960 and subsequent years should be sent to the University of Hong Kong Press, Hong Kong. Make check or money order payable to the Far-Eastern Prehistory Association in any currency that can be changed into Hong Kong dollars or British sterling. The rates for 1959 are: regular member, $4.00 U.S., £ 1/9 British, $25.00 Hong Kong; institutional member, $5.00 U.S., £ 1/16 British, $30.00 Hong Kong; student member, $3.00 U.S., £ 1/2 British, $20.00 Hong Kong.

For further information and payment of dues or subscriptions prior to 1960, write to the Editor, Wilhelm G. Solheim II, Department of Anthropology, University of Arizona, Tucson, Arizona, U.S.A.

▶ The BRITISH ETHNOGRAPHY COMMITTEE, founded in 1948, began in 1958 to revise and expand its membership. Two subcommittees have been formed; one on Social Anthropology and one on Material Culture. The former plans to compile a list of individuals and institutions engaged in social studies of British communities and a bibliography of published and unpublished works on the social organization of these communities. The second sub-committee is coordinating existing regional collections of folk life material, preparing a guide to them, and setting up a central archive on the subject. Write to Mr. J. G. Jenkins, Hon. Secretary, British Ethnography Committee, Royal Anthropological Institute, 21 Bedford Square, London, W.C. 1, England.

▶ The DANISH ETHNOGRAPHICAL ASSOCIATION was founded in 1952 to create a link between researchers and the Danish public. The activities of the Association were extended through the publication in 1959 of the first issue of an annual journal, *Folk,* intended not only for the Danish public, but also for scientists abroad. Its scope includes all branches of anthropology, in the widest sense of the word, with due regard to affiliated disciplines. Articles are in English, German, or French, according to the preference of the authors. Subscription price: 20 D. Kr. (1 English guinea, $3 U.S., 13 DM., or the equivalent in other currencies). Write Miss Inger Wulff, Secretary, Danish Ethnographic Association, c/o The National Museum, Ethnographical Department, 10, Ny Vestergade, Copenhagen, Denmark.

Carbon-14 Dates

▶ Associates who wish to obtain a "working compilation" of radio-carbon dates significant for Pacific anthropology may do so free of charge. Write to Miss Brenda Bishop, Secretary, PACIFIC SCIENCE ASSOCIATION, Bernice P. Bishop Museum, Honolulu 17, Hawaii, U.S.A. The list was compiled by Kenneth P. Emory and Yosihiko Sinoto of the Anthropology Department, Bishop Museum. It is mimeographed and was originally issued as a supplement to *Pacific Science Association Information Bulletin,* Vol. 11, No. 3, and is current, though not exhaustive in coverage, through May, 1959.

Free Catalogs

▶ . . . of films on Asia, a selected and annotated list, write to the ASIA SOCIETY, INC., 112 East 64th Street, New York 21, N.Y., U.S.A. The Society reviewed over 350 films in making this selection, covering the area from Afghanistan to the east and pertaining to various aspects of Asian life. Information about rental and sale of films has been given whenever obtainable. The Society plans to revise the list periodically, and requests information on new films, or those which have been overlooked. Single copies are available without charge from the Society.

▶ . . . of recordings of Pacific Islands music, in mimeographed form, write to Richard Seddon, Executive Officer for Social Development, SOUTH PACIFIC COMMISSION, Noumea, New Caledonia. Recordings on tape and disc from many parts of the Pacific have been collected, and are interchanged among broadcasting stations in the area for use in special programs for island peoples. A small number of discs from selected tape recordings made in the territories are available for sale.

▶ . . . of ethnographic 2″ x 2″ Kodachrome slide series for sale by ART COUNCIL AIDS, write to Ralph C. Altman, Editor, Box 641, Beverly Hills, California, U.S.A. The series includes such topics as Alaskan Eskimo arts (24 slides, $18.00), African sculpture (41 slides, $30.00), Australian bark painting (8 slides, $6.00), and the arts of New Guinea (34 slides, $25.00), photographed from original source materials in museums and private collections. The slides are intended for use in schools, libraries, and museums, and each series has an accompanying commentary describing and identifying the materials.

Wanted

▶ Information about cases in which research on a racial group, tribe, culture, or language is urgent in order to save data which might otherwise be lost, by the Editor of the *Bulletin of the International Committee on Urgent Anthropological and Ethnological Research,* Robert Heine-Geldern, INSTITUT FÜR VÖLKERKUNDE, Reitschulgasse 2, Vienna 1, Austria. Reports may be written in English, French, German, Spanish, Italian, Portuguese, or Dutch. The Committee has at present no funds of its own for the support of field research, but will do everything in its power to aid urgent projects. The *Bulletin* will be sent free to anyone interested, and the Editor wishes to obtain the addresses of scholars, government officers, missionaries, or others who might collaborate.

▶ Books on Zoroastrianism and Ancient Iranian culture, by the ARDESHIR YEGANEGI PUBLIC LIBRARY, 141 Villa Ave., Teheran, Iran. Founded in 1959 by Mrs. Yeganegi in memory of her huband, and planned as a center for research, the library is equipped to house 20,000 books, of which 4,000 have been acquired.

▶ Books and library materials on the anthropology of the Americas, by the newly formed SEMINARIO DE ANTRO-

POLOGÍA AMERICANA, Facultad de Filosofía y Letras, Universidad de Sevilla, España. The Seminario was founded by José Alcina Franch in May, 1959, and its research orientation includes ethnohistory and acculturation (principally through the use of the Spanish documents in the Archivo General de Indias in Seville), comparative ethnology, folklore, and archaeology. It will also concentrate on theoretical and interpretive studies of the history of primitive America.

▶ Information about recordings of the music of the St. Francis, Penobscot, Malecite, Micmac, or Passamaquoddy Indians, by NICHOLAS N. SMITH, Seminary Hill, Carmel, New York.

▶ Offprints or unpublished reports of general anthropological interest, particularly on South-East Asia and the Pacific, by the DEPARTMENT OF ANTHROPOLOGY AND SOCIOLOGY, AUSTRALIAN NATIONAL UNIVERSITY, Box 4, G.P.O., Canberra, A.C.T., Australia. The Department is building up its collections of such materials, and would be grateful for their receipt.

Salvage of African Wall Paintings

MUSEUMS which collect and exhibit the traditional arts of Africa may wish to secure examples of rapidly disappearing African house-wall fresco paintings. Dr. Herta Haselberger, Technische Hochschule, Institut für Kunstgeschichte, Karlsplatz 13, Vienna 4, Austria, is interested in undertaking an expedition for this purpose, and is anxious to contact interested museums. She reports that within five to ten years, the highly stylized wall decorations made by women will be extinct, since only a few artists of advanced age are still able to produce them, and the houses on which they now exist stand, at the most, about ten years.

Haselberger tells us that the art of painting was practiced in Africa long before contact with the Portuguese. Men and women decorated their bodies with colorful drawings for dance and religious purposes, and houses, temples, religious objects, and daily utensils bore painted decorations.

There are two main types of frescoes: geometric paintings made by women on their houses, and figure paintings made by men on the temples and chiefs' houses. The geometric decorations (see illustration) are found in the area along the 10th latitude in West Africa from the Bassigo archipelago to Lake Chad. Similar paintings are made in Southern Nigeria, the Congo Basin, and in South Africa. Design elements include colored circles, triangles, rhombuses, stripes, and checked patterns. These are everywhere the same, and are similar to those in Late Palaeolithic–early metal periods of the eastern Mediterranean.

The figure paintings fall into two groups, pictures with mythical and religious content being found on the walls of temples in the Northern Niger area, the central Guinea Coast, the Congo Basin, and in East Africa, and paintings representing scenes from everyday life occurring in areas where Western arts have had more influence.

Like the prehistoric paintings found in Europe and Africa, the figures in the paintings stand free, floating in an ideal surface that has been divided by carving or by coloring. Perspective, the concept of a horizon, and background scenery—these principles the artists have ignored for seven centuries. Where external influences have been absent, the colors used are almost exclusively white, red, and black. White is the color of spirits and of the dead. Red symbolizes rejoicing and is appreciated by the gods. For sacrifices to evil spirits, black is used. Use of other colors, especially of blue and green, is probably due to Egyptian influence, which spread in connection with Islam, as well as to European expansion. Paintings are done on a background layer, about two centimeters in depth, made of ant hills and cow dung, with dyes derived from plants, animals, and minerals. Kaolin-earth, chalk, manioc flour, mud, and the excrement of birds are used to make white paints. Red dye is made from red earths, red lice, and the bark of red trees. For black, black earth, graphite, soot, charcoal, and carbonated plants are used.

Paint brushes are made from leaves bound together, and frayed and sharpened at the ends. Some artists paint with small berries, or simply use their fingers. Flat stones are used to polish the completed work.

Haselberger would like to hear from any museum interested in entering into a contract to receive examples of this art, or other objects, in exchange for financing the removal of frescoes. Entire houses, decorated both inside and outside, could be collected and reassembled in museums, or particularly interesting sections, displaying figures and inlays of cowrie shells, could be obtained.

Conferences

Quantitative Methods in Archaeology

July 1–9, 1959
at Burg Wartenstein, Austria

Sponsored by The Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairman: Robert F. Heizer, University of California, Berkeley, U.S.A.

Participants, and their papers prepared and exchanged in advance:

Ch.-A. BAUD, École de Médicine, Genève, Switzerland. "Dating of Prehistoric Materials by Radiological and Optical Methods."

H. H. COGHLAN, Borough Museum, Newbury, England. "Metallurgical Analysis of Archaeological Materials."

S. F. COOK, University of California, Berkeley, U.S.A. "Dating of Prehistoric Bones by Chemical Analysis."

I. W. CORNWALL, University of London. "Physical and Chemical Analysis of Soils as Bearing upon Archaeology."

A. J. H. GOODWIN, University of Cape Town, South Africa. "Chemical Alternation (Patination) of Stone."

ROBERT F. HEIZER. "Physical Analysis of Habitation Residues."

W. W. HOWELLS, Harvard University, Cambridge, Mass., U.S.A. "Estimating Population Numbers through Material Remains and Documentary Sources."

F. R. MATSON, Pennsylvania State University, University Park, Pa., U.S.A. "Chemical Analysis of Ceramic Materials."

A. C. SPAULDING, University of Michigan, Ann Arbor, U.S.A. "Statistical Description and Comparison of Artifact Material."

H. V. VALLOIS, Musée de l'Homme, Paris. "Vital Statistics of Prehistoric Populations as Determined from Archaeological Data."

F. E. ZEUNER, University of London. "Geochronological Methods as Pertaining to Dating of Archaeological Materials."

Additional participants:

A. C. BLANC, Institute of Human Palaeontology, Rome.

J. G. D. CLARK, Cambridge University, England.

R. PITTIONI, Urgeschichtliches Institut, Vienna.

R. SINGER, Department of Anatomy, University of Cape Town, South Africa.

Publication: The papers and discussions are being prepared by Heizer for publication early in 1960.

Discussion:

On metallurgical analysis, Pittioni, the main discussant, spoke on the idea of "impurity patterns," by which, working with spectrophotometric technique, relative amounts of each trace element in metal implements and ores may be compared. On chemical analysis of ceramic materials, discussion revolved around the role of pottery in prehistoric societies, including the sexual division of labor with respect to pottery making, the estimation of number of pots from fragments, the manner in which vessels break into fragments, methods and functions of tempering, and the urgent need for fuller ethnographic descriptions of pottery making.

On statistical description and comparison of artifact assemblages, discussion centered on examples of techniques, and on the problem of sampling. Consideration of physical analysis of habitation residues again raised the sampling problem, and Clark presented details of the new proton magneto-meter which has proved of great importance in England in locating sites and mapping sub-surface features.

With regard to methods for estimating prehistoric populations, discussion emphasized the critical importance of the time factor in such estimations made on the basis of archaeological data. In the discussion of chemical and radiological methods of dating bone, fluorine and paper chromatographic analysis were emphasized. It was agreed that present methods provide only for relative dating, under restricted conditions.

Cornwall's paper on soil samples led to discussion of the use of micromorphological methods, the character of cave sediments, and the interpretation of fossil soils. Lively discussion on Goodwin's paper led to agreement that, although the phenomenon of patination is of use in archaeological interpretation, no dating system based on it is likely to be developed.

Zeuner reviewed new evidence and interpretation of deep sea cores, potassium-argon dating, dendrochronology in Europe, sources of error in radiocarbon dating, paleomagnetism, and thermoluminescence.

Results:

The conference concluded with the adoption of recommendations: that more intensive soil-morphological work in the study of archaeological deposits is needed; that agreement among archaeologists on standard terminology of types and descriptive statistical methods of analysis is desirable; that analyses of data may have greater statistical validity if adequate methods of sampling of archaeological materials were established; that a compact field manual should be prepared to improve methods of recording age and sex of skeletal material; and that coordination of European research on technology and spectroanalysis of metals is needed.

Stability and Change in Thai Culture

August 9–15, 1959
at Burg Wartenstein, Austria

Sponsored by the Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairman: R. Lauriston Sharp, Department of Sociology and Anthropology, Cornell University, Ithaca, New York, U.S.A.

Papers by members of the Cornell University Thailand Project, prepared and distributed in advance:

R. Lauriston Sharp. "Thai Social Structure."

Jane R. Hanks, Bennington College, Bennington, Vermont, U.S.A. "The Thai Family and the Life Cycle."

Lucien M. Hanks, Jr., Bennington College, and Herbert H. Phillips, Cornell University. "Socialization, Education, and Personality."

R. Lauriston Sharp. "Local Initiative, Government, and Administration."

Lucien M. Hanks and Robert B. Textor, Cornell University. "Religion, Idea Systems, and World View."

Other participants:

E. E. Boesch, University of Saarbrücken, The Saar, Germany.

Siribongse Boon-Long, Shell Oil Company, Bangkok, Thailand.

Anne Burgess, Health Education Specialist, Geneva, Switzerland.

M. Francois Geoffroy-Dechaume, French Foreign Service, Paris.

Charles Madge, Birmingham University, England.

Margaret Stepan, World Health Organization Consultant, Vienna, Austria.

Suppanee na Songkhla, International Institute for Child Study, Bangkok, Thailand.

Kenneth E. Wells, Presbyterian Mission, Bangkok, Thailand.

Robert Heine-Geldern, University of Vienna, Austria.

Publication: The papers are to be published in the *Cornell Studies in Anthropology*.

Discussion:

Agreement was reached concerning the fundamental features of traditional Thai culture conditioning the present and future: a stable, adequate technology in a predictable environment, providing good nutrition and a surplus for urban and population growth at a slow rate; a bilateral nuclear family structure permitting mobility and individual autonomy with higher loyalties to religion or the state; an open class system of linked hierarchical relations; a long history of political independence and unity supporting selective borrowing; a pervasive homogeneous religion and world view congruent with cultural forms and personality type; a stable personality type, of undetermined character.

On Thai personality, past and present, there was agreement that a Thai image of a modal type exists, but that research data are so far insufficient to permit solid conclusions regarding Thai psychology in depth, and that this is an important lacuna in considerations of changing Thai culture. For various aspects of Thai behavior some definition was achieved of mechanisms, past and present, in the physical and cultural environment which helped maintain or change patterns, including standard methods of socialization, informal and formal sanctions, and aspects of ideas and sentiment systems. It was recognized, however, that there were massive influences deriving from various levels of Western civilization (i.e., temporal controls; action and idea patterns associated with machines; scientific and speculative thought; attitudes toward sport, public service or welfare; and cinema and popular new musical, art, and literary forms) which have not been adequately studied and assessed.

Results:

It was concluded that to a remarkable degree Thai civilization is being modernized, particularly in rural areas, by processes of addition, substitution, and redefinition which offer no basic threats to older norms that traditionally permitted very great latitude and range. Reorientation occurs without alienation from tradition, and indeed, many traditions are reaffirmed.

The conference had only limited time to explore the implications of these conclusions for applied anthropology in specific situations or problems in Thailand today. A need was expressed for better comparative material from neighboring Oriental cultures as an aid in supporting conference conclusions on the unique "Thai-ness" or otherwise of both traditional and current behavior among Siamese, and particularly the peasantry.

Continued from inside front cover

ally publish whatever it is that Associates wish to learn from and to teach one another. An important part of the journal may be pages of direct appeals for specific items of information needed to pursue lines of research. Another may be discussion of ideas for research by scholars who do not themselves have time or opportunity to pursue them.

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